FARLOWIA

A JOURNAL OF CRYPTOGAMIC BOTANY

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IN HONOR OF WILLIAM GILSON FARLOW

DAVID H. LINDER

Although this journal is but two years or one volume old, it is nevertheless not too young to recognize the one hundredth anniversary of the birth of the man who, through the founding of the Farlow Reference Library and Herbarium of Cryptogamic Botany, made possible its existence. The occasion is of greater importance than the mere fact of the journal's existence, for Dr. Farlow was more than a means of making its existence feasible, he was the first professor of cryptogamic botany in America and played an important part in the development of this branch of botany. Not only did he stress the importance of systematic cryptogamic botany and morphology, but he initiated in this country the teaching of what is now known as phytopathology and he also began, with Professor W. R. Nichols, the study of the rôle of algae as impurities in drinking water. His versatility and his contributions have been ably and more amply described in the biographies appended to this paper and for this reason only a brief sketch is needed on this occasion.

William Gilson Farlow was born in Boston on December 17, 1844 and there spent the first fourteen years of his life before the family moved to Newton, Massachusetts, then considerably less urban than it is today. Perhaps it was the life in the country where he was surrounded on all sides by Nature's beauties and curiosities that stimulated his enquiring mind and influenced him to become a botanist or maybe it was the influence of his father, John S. Farlow, who although a successful business man, was equally enthusiastic about horticulture, as is attested by the prizes that he won at the exhibitions of the Massachusetts Horticultural Society and by the gifts that he made to that institution. Whether it was the father's interest in plants or his own discovery of the wild *Hepatica*, which has been said to have stimulated him to pursue botany as a life study, there can be little doubt that William Farlow received the encouragement and backing of his family, and that his father by example set the pattern that he was to follow.

While at Harvard College, from which he graduated in 1866, he continued his interest in natural history, taking the courses offered by Asa Gray and being elected president of the then active Harvard Natural History

Society. On graduating from college, he spent four years at the Harvard Medical School where he obtained his M.D. degree. He did this, even though highly successful as a student, not because he was primarily interested in medicine, but because in those days botany was the hobby of amateurs who were for the most part physicians and ministers and there was little opportunity for the professional botanist. To quote his words (Science N. S. **37:** 79. 1913) "When I graduated from college in 1866 and wished to become a botanist, Professor Gray told me that I ought to study medicine first because the possibility of gaining a living by botany was so small that one should always have a regular profession to fall back upon." . . . "In 1866, there were very few botanical professorships in this country, the salaries were very small and the equipment very shabby. Gray was professor at Harvard, D. C. Eaton at Yale and Porter at Lafayette. Torrey, in spite of his distinction as a botanist, really depended on his position as a chemist for his living. The comparatively few positions in government and state stations offered few attractions and changes were frequent. To a young man the prospect was not assuring."

Upon graduating from medical school, Farlow returned to botany, becoming Asa Gray's assistant. He began in earnest to pursue his studies of cryptogamic botany but after two years, found that if he wished to progress in this field, it would be necessary for him to go to Europe. Hence, in 1872, he went abroad and spent the two following years, for the most part in the recently founded laboratories at Strasbourg under the guidance of Anton De Bary, a brilliant crytogamic botanist whose scientific conclusions were often in advance of his time and whose teaching appears to have been most provocative. But despite the fact that he was learning De Bary's methods of teaching and research, he nevertheless, on the recommendation of Dr. Gray, took out time for intensive studies of the lichens with Dr. J. Mueller and of the algae with Thuret and Bornet. The trips, if we may judge by his letters, gave him an opportunity to visit many important herbaria in different parts of Europe and to make the acquaintance of the distinguished botanists of the period, acquaintances with whom, after his return to America, he maintained an active correspondence. His stay in Europe also provided him with an opportunity to obtain rare and important volumes for his library and equally important specimens for his herbarium to which he added while still abroad, the most important Curtis collection which contained parts of the types of species described by Berkeley and other specimens obtained by exchange with Schweinitz, Ravenel, Sprague, Fries, and Duby. Thus in spite of being small in stature, a fact about which Dr. Farlow was very sensitive, he was a giant when measured by his physical and mental activity.

Although worried about the prospects of a position as a professional botanist on his return home, when he did come back to America he was appointed through the friendly efforts of Dr. Gray to an assistant professorship and five years later, in 1879, became Professor of Cryptogamic Botany. During the early days he taught not only cryptogamic botany, but

at the Bussey Institution where he was stationed the first five years, gave instruction to special students in the applied side of mycology, probably the first to be given in that now thriving field known as phytopathology, to which he was then making contributions.

When he became Professor of Cryptogamic Botany he moved from the Bussey Institute to Cambridge where he apparently enjoyed greater freedom of action and where he continued with zeal to build up his already important herbarium and library. Although still devoting his attention to mycology, he devoted more time to the algae, his first love, and published among other papers, the "Marine Algae of the New England and Adjacent Coast," a manual that has been in use up to very recent days. Although interested in the morphology and taxonomy of the algae, for which his studies well prepared him, just as with the fungi, he saw the practical importance of the freshwater algae and published three or four papers which resulted from his study of the water supply problems of the City of Boston. Again, although primarily a systematic botanist, he was one of the first in this country to demonstrate the importance of the lower forms of plant life in the welfare of the people.

Little more need be said here on Dr. Farlow's reputation as a mycologist or phycologist since these subjects are more amply discussed in accompanying papers. Less well known were his interest in lichenology and bryology, perhaps because he published little on these two subjects. However, that he did much work in these two fields is attested by a perusal of the specimens in his herbarium where can be found a considerable amount of material that had been named by him, as well as the critical notes and observations that were written in his characteristic and occasionally not too legible hand. His remarkable memory and his very broad knowledge of these groups enabled him to provide information of great value to students and specialists alike, and his herbarium was available to those who were properly qualified. At times he has been criticized for not allowing the herbarium to be used unless under supervision. This was not because of selfishness on Farlow's part but rather the result of abuse by thoughtless visitors who ruined or even entirely removed some precious specimens from his collections. Farlow did not bring together the many specimens for immediate consumption and dispersal, for his endeavors were all directed towards building a reference collection that would serve future generations of botanists who were capable of profiting by what he had done.

One cannot but be impressed by the tremendous energy shown by Dr. Farlow. Although he gave up undergraduate instruction twenty-three years before his death, he continued to take a keen interest in graduate students. In addition to his teaching, he gave considerably of his time to answer correspondence and through it to maintain the friendships that he had made as he went through life; the administrative needs took no small portion of his efforts, and withal he did a tremendous amount of research in very diverse groups of non-vascular cryptogams. Yet somehow he found time to make collecting trips inside and outside of the country, to attend

meetings of the various societies to which he belonged, and to be present at the International Botanical Congresses. It is small wonder that he had little time for publishing the results of his own observations. In addition to the activities indicated above, he was also interested in founding an international journal in which could be found the names of new species of plants that had appeared in the scattered literature during the short intervals between issues, thus serving to lighten the bibliographic tasks of the systematists and applied botanists as well. As papers were being published at an increasing rate in widely scattered journals, it was even at that time a problem to keep abreast of the current literature and only by personally supporting an assistant and bibliographer was he able to maintain an index of his own covering the fungi that had been described from North America. This index, in 1919, contained some 300,000 entries and was available to colleagues for consultation. The reason behind all these efforts seems best to be explained by his own words (Science N. S. **37:** 7. 1913): "I, who was almost the first American student to seek the benefit of botanical instruction abroad, have lived to see the time when a very large number of botanists have brought back to America the best that Europe has to offer. There was a time when our botany might have been said to bear the mark 'made in England.' In more recent years it may be said to have been 'made in Germany.' There are some patriotic souls who hope the time will come, if it has not already come, when we may say 'made in America.' I do not share their feeling. To me it seems that botany is destined to become world-wide and it will be enough if we can contribute our proper share to the general stock." Dr. Farlow died in 1919 before the intense nationalism that grew up in the following years exploded into the chaos of our time. Let us hope that when the present conflagration is over, humanity may have learned the need of not only the scientific but also the political coöperation that he hoped to see.

While still a student in Europe, Farlow realized the dangers of the mad rush to every new fad that comes up in each generation to the neglect of systematic botany, for he wrote to Gray "I have been to see Prof. Pfitzer, who is a pupil of Hanstein, the man who doesn't think it any consequence to study the forms of common plants, but much better to mope over the 'Vegetationspunkt.' Dr. Hillebrand and I agree that, at the rate the Germans are going on, in twenty years there won't be a botanist in Germany who knows anything about the flora of the country." In many respects his prophecies were fulfilled, though at a slightly later date. At all events, he was sufficiently convinced of the importance of systematic botany to bend all his energies and a goodly portion of his means towards building up the library and herbarium to make possible the continuance of this line of study. He bought books, new and old, as they appeared on the market; he bought exsiccati sets and had one of the best collections of these in the country; he collected specimens during his trips at home and abroad and determined many specimens for his colleagues and thus gathered an herbarium that was very representative of nearly all groups of cryptogamic

plants. All of this he gave to Harvard University along with an endowment which, although not sufficient to maintain an adequate staff was, until the recent depression, enough to carry on in a very modest way the work that he had started.

Just as Dr. Farlow was a systematist who saw the importance of other biological fields and, indeed, helped to initiate them, his successor, Dr. Roland Thaxter was of much the same stamp and likewise continued to stress the importance of systematic botany when all young mycologists were stampeding into the rapidly growing field of phytopathology. A former student and later a very close friend of Dr. Farlow, Roland Thaxter continued his predecessors's policies, buying the important mycological herbaria of von Hoehnel, Patouillard, Burt, and Bucholtz, the lichenological herbaria of G. K. Merrill and of Sbarbaro, and the bryological collection of Fleischer and parts of the Stephani herbarium. To these have since been added the Sprague and Bartholomew herbaria of fungi and the Verdoorn herbarium of Hepaticae. These herbaria in addition to the Curtis collection of fungi, the James, Austin, and Sullivant collections of bryophytes, and the lichenological herbarium of Tuckerman, are rich in type specimens and accordingly very important to the systematist. As a result of the accretions over the years, the Farlow Herbarium is one of the largest, if not the largest, of its kind in America and rivals the few large ones in Europe. Today the library contains some 36,000 volumes, including periodicals, pamphlets, and reprints, most of which deal with the field of cryptogamic botany. It contains many rare and indispensable books of which some are not represented elsewhere in this country. The herbarium, with its many types or classical species, today contains 969,610 specimens distributed in the various groups as follows: fungi 529,885, lichens 119,315, algae 54,582, mosses 222,579, and hepatics 43,249. Thus the Farlow Reference Library and Herbarium of Cryptogamic Botany in collaboration with the Laboratories of Cryptogamic Botany, which have become separated and are now located in the Biological Laboratories building nearby, is in an excellent position to continue the tradition established by Farlow.

Since Dr. Farlow placed so much emphasis on systematic botany and showed his faith in the importance of the subject by devoting the greater part of his life and means to building up the institution that bears his family's name, it seems desirable to examine the standing of this phase of botanical science as it is at present. There is little doubt that with the opportunities opened up by plant and forest pathology, and to a lesser extent medical mycology, systematic mycology has been neglected, much to the detriment of both mycology and those related fields. The war has brought to light additional fields in the study of antibiotics (with penicillin as a shining example) and of deterioration of manufactured materials which, although a less spectacular field of investigation, is nevertheless important because it increases the cost of the war and thus effects every American through that sensitive organ known as the pocketbook. There is little doubt that these two new lines of endeavor — they cannot be called

fads since they are of tremendous immediate importance — are for some time going to drain young men from mycology, and that systematic studies, in the eyes of the public and even of scientists themselves, for they are not beyond the reproach of being shortsighted when it comes to recognizing a subject outside of their special fields, will fall into oblivion and accordingly will lose the meagre support that they now receive. We have seen how German botany, once virile, almost perished without systematic botanists.

How would the related fields of botany, or of chemistry or any other branch of science that deals with living organisms suffer? We have already seen in this war that when a person not properly trained is assigned to a primarily botanical task, it is not until the botanist with systematic training and with the accompanying techniques is called in that the essentially botanical problem begins to clarify and to yield results or to clear up inconsistencies that have previously been baffling.[1] Just as it is nearly impossible for a chemist to step in and solve a mycological problem (and vice versa) most effectively, so too is it when a chemist, a doctor, a physiologist or what-have-you tries to do monographic work or determine unusual forms of plant life. To do this needs the experience that the taxonomist has received through training and, even more important, through the experience of constantly studying a wide range of material and the necessary literature. The classification of organisms is but one phase of systematic work. Another phase is the collecting of material. Taxonomists are looked upon much in the same light as the pack rats, — never satisfied with what they have. This is rather praise than criticism, for it is only by gathering extensive lots of material from various localities and under different ecological conditions that it is possible to determine the range of variation of a species. Furthermore, it has been from the kleptomaniac instinct that our knowledge of the organisms that inhabit this green globe has increased, not infrequently to the great benefit in a practical way to the economics of a people, as is shown by the development of disease resistant strains of wheat. Whatever of an experimental nature is performed, it is fairly certain that the experiment cannot be repeated or of general value unless the organism has been accurately described and properly named.

Much more could be written concerning the importance of systematic botany and of the necessity of libraries and herbaria, to say nothing of financial support, for its proper and continued development. However, it is hoped that the present remarks are sufficient to emphasize that while immediate practical or economic application of knowledge is of great importance, nevertheless it is impossible to tell when the so-called useless knowledge of pure science may be called upon. It is therefore desirable that there be a balance of a sort between applied and pure science. This seems to be one of the lessons that we can learn from Dr. Farlow's life.

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[1] Emmons, C. W. — Misuse of the name "Trichophyton rosaceum" for a saprophytic Fusarium. Jour. Bact. **47**: 107–108. 1944.

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THE CORRESPONDENCE OF WILLIAM G. FARLOW DURING HIS STUDENT DAYS AT STRASBOURG

Hilda F. Harris

Ever Dear Father

From

Asa Gray

Thanks for your photo.
Very nice! How you must take
after your wife!

PLATE I

2(1): 9–37 FARLOWIA January, 1945

THE CORRESPONDENCE OF WILLIAM G. FARLOW DURING HIS STUDENT DAYS AT STRASBOURG

Hilda F. Harris

In June, 1872, Asa Gray wrote to his friend Alphonse De Candolle in Geneva, "My youthful assistant of the past two years goes in a week or two to Europe, to study in some German university for a year or two; to Strasburg, I think, unless he first should go to Sweden, and there study Algae, with Agardh, if he will receive him. He takes a fancy to lower Cryptogamia. His name is Farlow, an honest, good fellow. He will most likely be in Switzerland in the summer; and I shall give him a letter of introduction to you, whom he will wish to know. But take no trouble on his account, except to introduce him to Dr. Müller, from whom, as a working lichenologist, he could learn much." [1]

With this modest introduction, William Gilson Farlow set out to indulge his "fancy for Cryptogamia" in two profitable years of study and travel in the outstanding botanical centers of Europe. Farlow's interest was well placed, and received the whole-hearted approval of Gray, who was quick to recognize the abilities of his young assistant, and was eager to have him return home equipped to introduce the study of the lower plants into the Harvard curriculum. The story of Farlow's study in the laboratory of Anton De Bary is chronicled in an exchange of letters between the two men — long letters from Farlow, rich in newsy narratives of the activities at Strasbourg, and briefer ones from Gray, revealing his deep interest in Farlow's progress and generous with wise advice as to the most profitable plan of study. By direct quotation from these letters [2] it is possible to create a picture of this interesting period in Farlow's life in which he received the preparation which was to influence so strongly his botanical

[1] Letters of Asa Gray edited by Jane Loring Gray. **2:** 625. Houghton Mifflin. Boston. 1893.

[2] The letters from Gray and De Bary may be found in the Farlow Library; those from Farlow have been generously lent by the Gray Herbarium, and the writer wishes to express her thanks to the Director, Prof. Merritt L. Fernald, for permission to quote from them. The two Christmas letters from Strasbourg have been added recently to the collection at the Farlow Library by Dr. David H. Linder, whom the writer wishes also to thank for suggestions made during the preparation of the manuscript. Dr. Rolf Singer has very kindly contributed the translation of De Bary's letter from the German. In a few of the letters quoted, obvious errors in grammar or spelling have been corrected.

Plate I. A portrait of W. G. Farlow, sent home during his European trip. Gray acknowledged its receipt in his letter of March 23, 1873, of which the portion is here reproduced in which he characteristically remarked, "Thanks for your photo. Very nice! How your moustache sets you off!"

work in this country, and during which time he acquired the nucleus of his now famous herbarium and library.

Farlow's first months in Europe were spent in traveling, but even then he was anticipating the prospect of his winter's work, for he wrote on August 12:

> Everyone speaks in glowing terms of De Bary and I hope to be suited with Strasburg. Hofmeister has gone to Tübingen and it will not be far to go to see him if it seems best . . . I am anxious to get to studying. I enjoy roaming about very well but I like better to be settled.

It was not until October 6 that he wrote that he was in Strasbourg at last, and had met De Bary. "His appearance is not striking. He is small and homely and looks too young to have written as much as he has."

If this initial meeting with De Bary failed to come up to his expectations, Farlow's impressions of Strasbourg were even more disappointing. Strasbourg had surrendered to the Germans in 1870 during the Franco-German war, and Farlow arrived there two years later to find the city still filled with soldiers, the university enrollment shrunk to a mere 250 students, and the German language, which he had hoped to master thoroughly during the course of his studies, spoken poorly and with an "Alsatian twang." Farlow took an immediate and permanent dislike to the city itself. In his letter of October 6 he complained:

> I do not, now, think I shall be able to endure Strasburg longer than one term. It is one of the most disagreeable places I have ever been in. The weather has been horrible. I have been here, in all, ten days and not a single one at all pleasant and the weather cold and raw. I cannot keep warm. As for society, there is absolutely none. The university instead of having 1500 students has only 250 and no English or Americans . . . Prof. De Bary thinks Strasburg will be a very good place to learn German but I do not agree with him. The people don't speak German and there is no society here except military men with whom I am not likely to become acquainted. I am almost sorry that I didn't go to some northern city and study the language and let botany alone for some months.

As time passed, only a growing realization of the excellence of De Bary's instruction forced him to admit reluctantly to himself that he could not hope to receive such training in any other place. As he said later (April 23, 1873) after a visit to Heidelberg:

> [It] is certainly one of the loveliest places I ever saw and is particularly beautiful in spring when all the trees are in flower. I was glad to come here because I wanted to find out, for myself, whether it is really a good place to study or not . . . Accordingly Veni, Vidi, but *not* Vici. I find that H. is not to be compared with Strasburg for botany and I leave tomorrow for the latter city to stay during May and June with De Bary.

Farlow has described conditions as he found them at the University of Strasbourg in a short paper which he wrote in October, 1873, intending it

for publication in the series of articles which appeared in the American Naturalist. In it he said:

The University of Strassburg which has, for two years, been under the control of the Germans, stands, as far as botany is concerned, in the first rank. It is rather the fashion to say that the University has been created by the Germans. This, however, is not true, unless we adopt the view that nothing can be a University which is not conducted on the same plan as those of Germany. Before the Franco-German war, the name of Academy was given to what is now the University, although the number of students was then much larger than at present. Strassburg has for a long time been the residence of distinguished scientific men. Of the botanists of the old régime, Schimper and Buchinger still remain. Millardet, the most promising of the younger men, is now in Nancy. Prof. Schimper, perhaps the most distinguished member of the scientific faculty, still gives lectures on fossil botany and has a very large and rich collection of fossil plants.

The active botanical work is performed by Prof. De Bary and Ass. Prof. Solms von Laubach with Dr. Fritz Sc[h]mitz as assistant in the laboratory. The number of students attending botanical lectures, if we except the course on Medical plants, which must properly be included in the medical department, has never been greater than fifteen and quite frequently not more than five. The attendance has sometimes been as low as two. I give these numbers simply to show how different are the conditions from those of our own universities. An American professor would hardly think it worth his while to deliver a lecture to five students. The different proportion of instructors and students affords the clue as to how the Germans are able to do so much high scientific work. The professors have very little else to do. It is for this object that they are appointed professors while in America the professors are only able to do any purely scientific work in intervals between long courses of elementary instruction which is done in Germany in the gymnasium.

Farlow began his own studies at the laboratory in October, 1872, and he described his work in two letters to Gray.

Strasburg, Oct. 6.

Dear Sir;

. . . I have now been in Strasburg five days in my rooms, up four flights, in the Hotel Allemannia. Every day I go to the laboratory where there are three other students besides myself: a young man, quite young [Rostafinski] who is a genius. He draws and paints very well, and is now at work on a book on the Myxomycetes; another, a native of Strasburg [Gilkinet] who is skilful with his fingers and a third, somewhat older [Suppanetz] from Gratz who is doing something or other with Pediastrum and some of the Desmids. The first two students have been with De Bary three years.

Prof. De Bary is a very good teacher. He is very thorough. He put me, at first, on to Chara and makes me draw everything with a camera. I find it rather slow work, and am afraid I shall have to stay two or three years with him to learn much, which is longer than I think I shall be able to. Here every one thinks that a knowledge of the larger fungi or algae is contemptible. That the only thing worth living for is to study the development of some of the lower forms. A sys-

tematic botanist of Fungi or algae is regarded as a shallow person, of course, without any ability . . . You will be surprised to hear that I have come to the conclusion that England is, on the whole, the best place to study as they have not such a mortal dread of anything systematic. I should like your advice about what I should study.

W. G. Farlow. Hotel Allemannia.

Strasburg, Nov. 1st. 1872.

Dear Sir;

. . . I have now been a month in the laboratory but the lectures did not begin till this week. The lectures I have decided to attend are, De Bary's on Vegetable Anatomy and on the Lower Forms of Life, Graf Solms on Cryptogamia, and Schimper on fossil botany. Each lecturer lectures three times a week. Graf Solms has the uncomfortable hour of 6 to 7 in the evening. None of the lectures begin till a quarter past the hour. When it is light and I am not at lecture, I work in the laboratory, with the three students that I mentioned in my last letter. At first, I had the development of Chara and, then, started on vegetable anatomy, first, with the sieve cells, which are interesting, and, now, I am studying the milk ducts in Euphorbia, a tedious but not uninteresting work. There is a new monograph on the latter subject by David, a pupil of Cohn. The probability is that I shall devote six months to anatomy and then take the Myxomycetes. At first, Prof. De Bary gave me as much attention as I could desire, but, lately, he has been entirely occupied by his duties as Rector in matriculating the students. He told Rostafinsky that he should soon be able to devote considerable time to me, which will please me very well, as I don't like his assistant Dr. Schmitz very well.

De Bary is an excellent instructor and a very pleasant man. The students all like him. His German is a little peculiar and it is hard to understand him but he speaks very good English. He has not much mechanical or artistic skill but he is business like and doesn't poke like most of the others. As a lecturer he is poor. He doesn't speak distinctly, doesn't draw well, and is not at all interesting. Here, there is nothing to learn in the way of addressing an audience. The Professors have a very careless way. De Bary's audience consists of five persons. Graf Solms, as he is called, doesn't speak English at all. I understand his lectures very well and they are interesting. He seems to be a very industrious man and very well trained in microscopic anatomy and the general structure of the lower orders. I should hardly think that he knew as much about flowering plants as was necessary for such a position as he has. He also lectures on Pharmakognosie but I don't attend. His delivery is somewhat better than De Bary's but still he is not a model. He has an audience of six . . .

On Wednesday I was matriculated. The imposing ceremony consisted in listening to a short poorly delivered address from Prof. De Bary, who is rector, then shaking hands with him, and receiving a ticket of admission. There are, now, about three hundred students in the University. I have to pay for lectures and laboratory 120 fr., not including Prof. Schimper's lectures which have not yet begun, and for which I have not paid. Schimper is, it appears, a fine zoölogist and geologist as well as botanist. He is attached to the zoölogical department . . .

Yours, in haste,

W. G. Farlow

Farlow had not been long at Strasbourg when the opportunity arose for him to acquire, for his personal herbarium, his first famous collection of fungi. Before he left America he had discussed with Gray the possibilities of buying the Curtis Herbarium, and in October he received the following note from Gray.

Cambridge Oct. 16, 1872

My Dear Farlow

We have much enjoyed your letters — the last from Berlin, & including several of the Linné photographs. They are *most delightful*, and I beg you will keep up the habit of so writing to me. When I can I will write to you in return . . .

When you left you were a good deal disposed to acquire the late Mr. Curtis's coll. of *Fungi*. Mrs. C. is now ready to divide the Fungi from the rest and to take *$1000*. I have to-day seen Mr. Sprague of Boston who advised my securing them, and offerred — long ago — to pay $500 towards it. He tells me that he is now in different circumstances from what he was, and wishes to forego the purchase — don't think he can afford to pay $500 at all. And putting in the *steam*-warming here has used up the means of the Herbarium for last year & much of this year.

So, if you are still very fungously inclined, and want to be *set up* in this department, I am out of the way, and you can buy the collection if you will. What say you? If you decline, I shall offer it to the British Museum.

Ever, dear Farlow,
Yours sincerely,
Asa Gray

Farlow replied promptly (November 1):

As to the Fungi, I have concluded to take them. The price seems to me to be a fair one and, although I know nothing about fungi now, I hope to some time hereafter. The collection is unique and certainly should not leave America. I think it will be worth a thousand dollars to me as a recommendation in securing a position when I return. It is something to possess the finest collection of fungi in America and I hope, before long, to have a good collection of algae, certainly the best in America . . .

I will write, at once, to my father about paying you for the Fungi and will ask my brother to call and see you about the payment. What shall be done with the collection? Had I better have it sent to my father's house? How can it be forwarded to Boston? I will ask my brother to settle these questions with you.

Cambridge Nov. 25, 1872

My Dear Dr. Farlow,

I received your letter — saw your Father, same morning — wrote to Mrs. Curtis — and now have this reply: —

Will attend to all . . .

Ever yours,
A. Gray

Gray enclosed in this note the letter from Mrs. Curtis.

Wilmington. Nov. 22nd. /72

Dear Doctor Gray,

I have this morning received your last letter, forwarded to me from Hillsborough, and hasten to reply, lest you should imagine me doubly neglectful. The former one, advising us to pack and forward to you, the books relating to Botanical science, left by my dear Husband, was received a few days before I left for a visit to my friends in this place, and it was scarcely possible to write just then.

I shall return home in two or three weeks, and will try to pack the Fungi, as you wish, although it is a subject for regret to all of us, that the Collection must be separated. When we have all arranged to send off, I will communicate with you again, and let you know in what manner to make the payment.

With the greatest respect and regard, I am, dear Sir, Yours,

M. De R. Curtis.

Farlow acknowledged Gray's kindness in offering to look after the collection, and in the same letter expressed his rather dubious opinion of De Bary's way of running the laboratory.

Strasburg, Dec. 19th.

Dear Sir;

I received your letter with the accompanying one from Mrs. Curtis a few days ago and am glad that you have consented to see whether the collection is in proper order although, I am afraid, that it may take more time than you ought to give. The collection of M. De Brebisson is for sale in fragments and I have ordered Cape and S. American algae to the extent of a few dollars. His collection of diatomes is very good and is supposed to be worth 2000 Thalers. A Dutchman I believe is going to buy it.

. . . As a whole, the laboratory looks about as much like a chemical laboratory as a botanical, since there are any quantity of bottles of reagents, test-tubes, evaporating dishes and a great hood. There is an immense amount of strong 40 p.c. alcohol used as everything, even algae, are put into it at once and we have each a small wash bottle full to moisten our sections every few minutes.

In some things they are deficient since there is only one drawing prism and one Redresseur and, what is abominable, not a single dissecting microscope.

On Monday next, begins a recess of a fortnight and I am going to Frankfort for a few days to rest, and, then, shall come back to study. I have now been here three months and can form an opinion of the merits of Strasburg. As far as the town itself and social advantages are concerned, it is detestably bad. It is a very bad place to learn any language and there is no redeeming feature about it. As far as the botany goes, sometimes I am satisfied and sometimes not. From what I learn from the other students, what I regard as deficiencies in the instruction are common to all Germany and not peculiar to this place. Everything is slow to me, but, then, I am in Germany. There is no *plan* of instruction whatever. That is, I must know what I want to study and ask for the material myself and go to Prof. De Bary as I would to a dictionary, for information. I have learnt, as have all the others, not to ask Dr. Sc[h]mitz questions, as the answer is more perplexing than previous ignorance and, when he comes round to ask what I am about, I tell him I have material enough to last for some time. I never let him

know that I want something more for, in that case, he picks up the first thing that comes handy whether it has any reference to my previous studies or not.

About eleven, Prof. De Bary comes into the laboratory for a little while and, generally, again late in the afternoon. He knows a great deal and is very pleasant but is very often interested in abstract propositions which no one but a German cares anything about. A fortnight ago Dr. Schmitz gave me a Lycopodium Selago to study and, after making sections and preparations of one kind and another, I thought I knew it pretty well when De Bary asked whether the capsule arose from the stem or the leaf. After two or three days' work I managed to get sections which satisfied him and, of course, myself that it arose from the leaf. Then came the question how it arose which he didn't know or, as far as I can find out, anyone else. After much more labor, I discovered that the epidermal cells begin to divide parallel to the surface and it grew out in that way. Then came the, to me, transcendental question what might the subepidermal cells do, and, for a week, I made section after section of the Vegetationspunkt with no result whatever till I used up a great bottle of material which put a stop, fortunately, to further investigation. I ventured to suggest to my neighbors that I thought, as I wasn't to stay in Europe forever, that my time might be better employed than searching for a needle in a haystack which I could do as well at home. My Alsatian friend, from whom I learn more than anyone else, said Oh! in a few months you will be able to write a monograph on the subject. That remark was a clue to the whole thing; they study for the sake of writing monographs, just what I don't wish to do here. I can do that when I get back . . .

Remember me to Mrs. Gray, and all the gentlemen in the herbarium.

Yours, respectfully,
W. G. Farlow

Farlow was always on the lookout for books as well as specimens which would provide him with a good working collection when he returned to America. He browsed through every available bookshop and catalog, and gradually accumulated for himself and for Gray an enviable collection of the best botanical books in Germany. By the summer of 1873 he wrote that he had a trunkful of books which he would like to send home, and the following summer he observed with some pride that as an added qualification for a position he had "the Curtis Fungi and the finest collection of algae and algological books in America and a good collection of modern works on fungi." Many of his letters to Gray contained reference to books which he had seen on the market, as well as observations on the usefulness of some of the newer works.

[December 19, 1872] Books are uncommonly dear, it seems to me, and we get German books in America very nearly as cheap as here. Among books which may interest you, are Thomé's of the Real Schule, Cologne, Lehrbuch der Botanik. He has also written a Zoölogy and both books are the standards in German schools. In the Botany, the plates are very good and he goes into the anatomy and Cryptogamia much farther than in our own text books, and, I think, too far for common school scholars. But that is a German failing. The third edition of Sachs Lehrbuch has just appeared. The principal change is in the article on Aleuron. The Lehrbuch in Germany is a sort of botanical Bible and there is no use in telling a German that there is a great deal in it that is stupid

and had better have been omitted particularly as it is borrowed from Hofmeister. A book which I am going to buy sometime and which would be a great help in your lectures on Economical Botany is Berg & Schmidt's Officinelle Pflanzen, in four moderate sized volumes with very good colored plates. It costs, I believe, 26 Thalers, a pretty high price.

[January 4, 1873] A book which I shall buy for myself and which, certainly, ought to be in the Cambridge library is Tulasne's Carpologia Selecta Fungorum, of which, if I remember rightly, you have only the volume on the Tuberacei. It is expensive, coming to, at least, fifty dollars, perhaps more. Another work, which I, possibly, mentioned in my last letter, is Schimper's Paleontology in three volumes, in French and horribly expensive considering. It is 30 fr. a volume, I was told. Strasburger's Coniferae & Gnetaceae is regarded as a good morphological work, that is, a book which nobody ever finds time to read.

The algae of De Brebisson which I ordered were previously engaged. I think, before I come home, I shall get Fuckel's Fungi Rhenani and Rabenhorst's Fungi Exsiccati unless they are already in Curtis's collection. Have you Rabenhorst's Characeae? I have been advised to get them. Dr. Schmitz has sent you his article from the Flora and I asked him also to send Mr. Wright a copy.

I spent Christmas week in Frankfurt stopping at the Hotel du Nord but spending most of my time at the Consul's Mr. Webster, father of the only real American in Strasburg besides myself. I went into the store of Joseph Baer and spent some hours looking over his books. He has a Jacquin's Flora Schönbrunnensis, in good condition for 68 thalers, a Hooker & Greville which Prof. Eaton may like. I bought Martius's Plant. Crypt. Brasil for $18, Mohl Vermischte Schriften for $1, Boerhaave's Historia Plantarum Lugduni-Batav. 1731, Dillenius Cat. Plant. Circa Gissam 1719, for 50 cents, the two last simply as curiosities. I found a Vegetable Materia Medica of the United States by Barton, Vol. 1 for $1.50 and bought it. I don't know whether a second volume was ever published. Michaux's Flora Am. Bor. 1803, 2 vols for $3.00. There was a Barton's Flora of the United States 1st vol. complete, and second not bound and not certainly complete for about $10. I couldn't find out how many plates ought to be in the second volume. If I had been sure that it was complete, I should have bought it for you as I heard you say you wanted it. In Mrs. Curtis' letter she mentions sending her husband's cryptogamic books to you. Have you bought them for the library or is there something which I ought to purchase?

If I felt sure of getting a permanent position at Cambridge, I should limit my purchases of books to those which, as far as I know, are not in the library at Cambridge. But, as it is, in consequence of losses by the fire, I suppose the college will not feel like increasing the number of its instructors for some time to come, so I must buy books which will be necessary if I go to a place where the library is not as well stocked as at Cambridge. The same feeling interests me in collecting. I don't want to preserve the phaen[er]ogams which I may collect but the knowledge that I have a collection of flowering plants will help me in getting a position in some other place than Cambridge. The fungi and algae, of course, I save and perhaps I shall the mosses . . .

[March 6, 1873] Last week I saw a catalogue of Moser in Tübingen with some excellent things remarkably cheap. I wrote at once but found the best things gone to my regret. I bought several pamphlets, on algae principally, and an Endlicher. I saw a copy of Sprengel's Das Entdeckte Geheimniss, which I know you wanted, but it had been sold unfortunately. I agree with you that single odd

volumes are a delusion and a snare. About De Bary's work every day something of his which I have never seen before turns up. As I expect to be here next term I shall secure more of his pamphlets for myself and next week I shall buy some of those for sale. Perhaps, when the photographs arrive will be a good time to suggest that he should send you a complete set of his works. Brefeld's Penicillium has just arrived in manuscript with ten plates said to be very interesting. Strasburger's Azolla is like all his works but I don't believe you will want it. Rüssow, a Russian, has written a book in German on the structure of higher cryptogams which I may possibly buy but I don't think you will want. Have you Schwendener's Flechten-Thallus?

[May 12, 1873] I have just ordered and secured! Sprengel's Entdeckte Geheimniss which I know you wanted. At last, I have got hold of a Postels & Ruprecht for 32 thalers in Berlin.

[June 30, 1873] A copy of Corda's Icones Fungorum is for sale and De Bary has told me that it is an indispensable work and advised me to buy it although the price is 130 Thl.

[November 15, 1873] Ruprecht's Tange, I bought before I left Cambridge. Eaton wants to use my big Postels & Ruprecht until my return and I shall write to my father to mail it to him. I have lately bought a few books and shall order more. I expect soon a Tulasne for 60 Thl. Two indispensable and unfortunately unprocurable books are Corda's Icones Fungorum and Sowerby's British Fungi. A Corda was sold last year for 130 Thl. Is there a Corda in America? Sowerby is in the Bost. Nat. Hist. I now have a good many pamphlets which have cost a heap of money. However, most of them are very useful and not to be found in America. Is the set of Pringsheim's Jahrbücher in Cambridge complete? I shall try to get De Bary to let me have his duplicate set, not quite complete, of Fuckel's Fungi.

[June 13, 1874. London] De Bary has bought Duby collection. I have bought Sowerby's Fungi and a Harvey for my own use. I can't make up my mind to give $200 for Kützing's Tabulae Phycologicae. I feel more like giving $80 for a Bulliard which is very useful for fungi and not often to be had.

After Farlow found himself more or less oriented to the German way of research, he began to shape up more definitely his own plan of study. His language difficulties, coupled with his intense dislike for Strasbourg, brought up the question of whether to stay on with De Bary, or to move to some more congenial city. Some of his problems were settled when, early in January, he received a number of suggestions from Gray who, kept at home by an attack of influenza, found himself with plenty of time for planning.

Cambridge Jany. 10, 1873

My dear Farlow,

Yesterday came another of your welcome letters — to solace me in *illness*: for I suffer with a dreadful influenza — & Goodale is away, at Baltimore.

While lying sleepless I have thought over your studies &c —

We are going to have the Cryptogamic Flora of U. S. done up, pari passu with the Phanerog. Sullivant is *engaged* to do the *Mosses*, in a volume — to which I hope to have Austin do the Hepaticae.

I want you to come home prepared to do —

1. The Algae — one vol.
2. The Fungi —
3. — since they are only *Algae & Fungi!* either dwelling together — or the lamb inside of the lion — you will probably have to do the Lichenes! Unless we can get them out of Tuckerman, in an intelligible form — which is doubtful. He is somewhere among your friends the *Germans*.

I should think you are doing pretty well — learning what you ought to know — getting the ways of working. But tell De Bary that if you stay there another *semester*, you must be put at work on & be taught *Fungi*. Of course, only low Fungi could be noticed there — or could be worthy of notice by your philosophers, such as the mould upon a Strassburg cheese or a bad-smelling sausage: *Wurst* — I think they call them, which is descriptive when spelled with an *O*.

If De Bary can't see that you are taught this special lore, why, I would migrate in due time . . .

I doubt if Hofmeister has ever been much of a teacher. Yet I always thought much of his work. I suppose your best chance would be with Nägeli. If they find *him* intelligible, there need be no despair anywhere.

Tubingen should be a cheap place to live, and a complete University town. Hegelmaier who worked up Lemna & other low Pha[e]nogams is a *Botanist*, and I should think you might learn much of him, as to ways of research.

Suppose you look in at Tubingen & see a little for yourself.

Munich is cold enough in winter, but an attractive town. *Nägeli* conducts a well-ordered botanical laboratory. Radelkoffer, his aid is a jolly good-natured man, who knows plants & flowers somewhat, as well as vegetable anatomy. So, I should be content with Munich for one trial.

But I have a capital idea for you when your summer vacation comes — Dr. J. Müller, Candolle's curator, at Geneva, is one of the best *practical* workers at *Lichens* &c. — the very man to initiate you into *Lichen genera* & Lichen work. When summer or spring arrives go up there, and arrange with him to take you in hand, & have with him, & De Candolle, & Boissier your respect raised for *Botany*. In 3 weeks of work with Müller you would learn *Lichens*, and Geneva is the place of departure & return for the Swiss excursions you will enjoy . . .

They are going over the collection of Fungi, Mrs. Curtis & son, — and in reply to a letter, I insisted that they were to see that all was thoroughly poisoned or repoisoned!

Get as many of the Lehrbuch's as you think worth while . . .

Oh — as to *Cohn* at *Breslau*. I think you might do well with him, for a while, as you say. And then, if Goeppert is not too old you might really learn somewhat of *fossil-plants* — which you never would from Schimper's lectures.

But I have another good idea. Get *Schimper* in a series of private lessons, as the days get longer, to put you up to *Musci*.

He is an old & famous hand at that, — and you might learn all his *wrinkles* in a fortnight.

Mrs. Gray is as much broken down as I am — & suffers with, what Dr. Wyman assures us is Pelliosis rheumatica! — I, with *influenza diabolica!*

Always Dear Farlow,

Yours truly

Asa Gray

Farlow replied on January 27.

. . . I am very sorry to hear that you and Mrs. Gray are sick. I had hoped that, with the better heating of the herbarium, you wouldn't be liable to attacks of bronchitis as before. However, I suppose, you are glad that Mrs. Gray is sick, since misery always likes company.

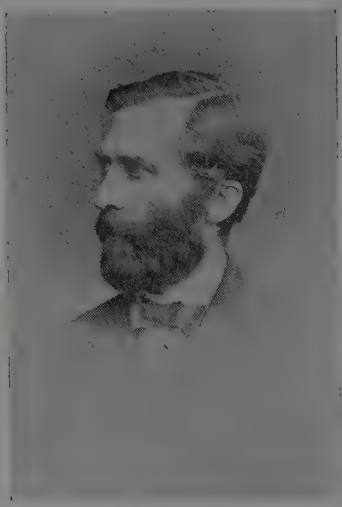
I was glad to get your advice about my studies. In a week or two I shall have finished my anatomy and shall then go on to Pilze. So far, my regular study has been anatomy but I have made occasional excursions into other branches. I learn a great deal, also, from the studies of the other students who always show to me what they have interesting. Stahl is at work making a lichen out of a fungus and an algae. I have now saved about 70 microscopic preparations which will be of very great service to me as I know exactly what points each specimen illustrates. We are taught to make very thin sections, no matter how small or ragged they look. I think I could stay with De Bary another term to study Fungi and Conjugatae and lower algae. Strasburg itself, however, is utterly unendurable and I find it fearfully hard to learn the language here. I had some thoughts of going to Bonn to spend the summer semester and study botany only forenoons and the language afternoons and evenings. I must learn the language at once. If I was in a private family and could give my whole time to German, I could get it. As it is, my best energy is spent entirely on botany. Your letter however, changes my plans. I must decline going to Tubingen or any other small town in South Germany until I have learnt the language better. I hope to study in Munich next winter. Your statement of what I am to learn hardly comes within the bounds of your usual modesty. I think I could stand the algae and, in course of time, the fungi, but, when you come to add the lichens, I begin to despair. I should like, as you propose, to study in Geneva and should like a letter of introduction to Mueller if you feel inclined to give me one.

As the term neared its end Farlow was able to estimate the value of the courses he had studied, and he seemed suddenly to find a surprisingly large number of reasons for enjoying his work at Strasbourg. On March 6 he wrote:

. . . For myself, the most interesting thing at present is that the semester will end next week and we have a recess of about six weeks, till May 1st, when according to all appearances I shall return to Strasburg for the months of May and June . . .

I like De Bary better and better the more I know about him and the students in the laboratory are pleasanter than I can again expect to meet in any one laboratory. Rostafinsky leaves and goes to Munich where I shall meet him again. Suppane[t]z also leaves to secure an appointment in Gratz. Döbrach goes to Bonn, but he was by far the least interesting of all. Gilkinet and Stahl remain but go with me to Munich next winter. Besides the botanists, I have only one acquaintance in Strasburg of any account, that is Webster, the American law student.

As to my studies (in botany not German) I suppose I am as near satisfied, as I, who am a first class grumbler, can expect. I have been over anatomy practically and by lectures very much more thoroughly than ever before and there is but one point which I have omitted which can be better studied in summer, the development of the embryo. I have about a hundred preparations and some



Anton De Bary

Ernst Stahl

Alfred Gilkinet

Josef Rostafinski

Plate II

drawings which will be of use to me and what is most important have learnt the German way of work and managing a laboratory which is radically different from ours. Lately I have been at work on the Pilze and their development in which branch De Bary has more practical knowledge than anybody else in the world. He is very pleasant and takes an interest in his students and gives them a reasonable amount of time and attention. I have studied the potato disease, Mucor and the allied forms, Piptecephalis and Chaetocladium, Ascobolus and Eurotium as types of Sphaeriaceae and Dictyostelium mucoroides. The last was particularly interesting from sowing the spores till the ripe sporangia were produced on the object glass. De Bary spent an hour showing me the original preparations of Brefeld's Mucor and Hallier's cholera Pilze, the latter very shabby, dirty and composed principally of striped muscular tissue. Incidentally I have examined other fungi, Uredineen &c. My neighbors in the laboratory are very obliging and help me in any way they can.

Next term, De Bary has two courses, a course on general anatomy which I shall not regularly hear as it is too simple, elementary, and once a week on the important medical plants. Graf Solms has two courses on Systematic Botany which I shall attend, if they are good, and on Phaenogamic Parasites. With regard to Schimper I don't think I care to study the mosses with him; he is learned but now old and a little tedious. I could learn nothing from him in a short time. I may instead study lichens with Graf Solms one or two afternoons in the week as a relief to the Pilze study. With De Bary I shall study only fungi and their development and leave the algae for Munich or, if I am so lucky, Thuret . . .

I have taken tea twice at De Bary's and a few Sundays ago all the botanists were invited by Gilkinet, the Belgian to dine. De Bary was there and we sat down to the table at one and rose at half past nine, a fearful feed. At the beginning each guest was provided with two enormous bottles of wine, and, when these were disposed of, more was brought on . . . Next week, I believe, Rostafinsky is to give a dinner.

Farlow derived a great deal of pleasure and no little amusement from his contact with De Bary's assistant, Graf Solms von Laubach. He described him as "a strange man, good hearted and well meaning but very awkward and, as a Graf, filled with strangely absurd stories about America." The Graf, however, was not the only one to have extraordinary ideas of America, because Farlow remarked in one letter "I am not unfrequently asked whether our lectures are in English or German by intelligent people. I have, also, been asked if it wasn't very pleasant collecting near Boston because the Gulf of Mexico must be a very good place for sea-weeds." At another time he told of meeting Müller's young son, who had recently been reading Cooper, and was "delighted to see some one who could tell him all about the Indians of Boston." Unfortunately Farlow did not elaborate upon the Graf's conception of this country, but he has described how he enlivened the work at Strasbourg. In a letter to Mrs. Gray (October 27, 1873) Farlow told of a collecting trip to which Graf Solms contributed most of the entertainment.

PLATE II. Contemporaneous portraits of Anton De Bary and three of the laboratory students with whom Farlow associated at Strasbourg.

Day before yesterday we all went on an excursion to the other side of the Rhine to a village called Kork. First, of course, we had to have some dinner and Graf Solms, who is one of the most awkward and amusing men I ever saw, began by upsetting his soup all over his clothes which was followed by a volley of Donnerwetters and wailings over grease spots which never can be taken out, requiring all the skill of De Bary to quiet him. The rest of us were so convulsed with laughter that we couldn't say or do anything. We all ran for the train and had to pay a fine of 50 ct. for coming so late. Kork is a flat plain interspersed with mud puddles and ditches and here the Kork youth with woollen night caps watch large herds of swine and geese. We proceeded slowly through this charming region the only question as to how wet we should get. At length we came to a very dirty pool full of different species of Elatine and Graf Solms began digging in the mud and screaming every time he fished up an Elatine to the great delight of all the swineherds and De Bary who called the plant Elatine spectaculosa in consequence. Another pool was full of Marsilea and the botanists all lay down flat in the muddy grass and began digging up the mud with their fingers and my umbrella, Graf Solms screaming at intervals and jabbering mixed French and German. When we were all dirty enough to go home, he discovered that he had not worn his excursion shoes which distressed him exceedingly. His troubles were drowned in a glass of beer at the depot.

As to his lectures, Farlow said (February 4, 1873):

Graf Solms I don't like particularly well on the Cryptogamia. He is now on the algae, of which he has made a perfect botch. There is no classification of the algae to be allowed because the 'Entwickelungs-geschichte' isn't well enough known; therefore, by German logic, Graf Solms is not to have any system in his lectures, in which negative point of view his lectures are a perfect success. Today a specimen of Gracilaria was passed round to illustrate Gigartina compressa and Wormskioldia was described as Delesseria. Porphyra was also in Florideae. The mixture was so bad that I don't wonder that the students here, not one of whom has ever seen the sea, think that the algae are in a wretched state.

Later he wrote (March 6, 1873):

Lately he [Graf Solms] has been lecturing on the Pilze and as his lecture room is next to De Bary's private laboratory he was afraid De Bary might hear his mistakes so that he has lectured in a whisper for some days to our great amusement. His greatest fault is that he has no system about his lectures but perhaps that is because he is an aus[ser]ordentlich[er] professor.

There was some thought at this time of calling a European botanist to Harvard on Gray's retirement, and this possibility made Farlow wonder about his own prospects after his return to America. Fearing that Harvard might not have a place for him, he asked Gray about the opportunities in California, saying that while he preferred to be in Cambridge, he was not "of the number who believe that all colleges but Harvard are barbarous." Gray, in his letter of February 23, 1873, hastened to reassure him. He wrote:

Take your time — prepare *thoroughly*. Meanwhile, if you get in a hurry — and if, as is likely any other place turns up — and places are sure to turn up — I will see to your interests all round.

Do not be at all uneasy. By the time you are ready there will be some good place for you. Anyway as soon as you come back there will, at the least, be an assistant-professorship here for you, I cannot doubt.

But your winning card, as it is what is *most wanted* in U. S. is Cryptogamy — especially low Cryptogamy. *Fungi* will be the most telling card. There you will have an exhaustless and a *popular* field — in which, well prepared — you can make a mark here.

Algae — are well, but marine soon exhausted . . .

Then *Lichens*. Tuckerman is hors de combat, I fear. You must really go into them! And, indeed, when Sullivant goes off the stage — which *must be* before very long — there is no fit moss-man.

Whether you should stay at Strasburg longer I am not competent to advise. I should say stay if Strassburgh were a pleasanter residence. You are just now entering upon the field in which De Barry can teach you much, & I hope would do so. But you must take the universities in their turn.

Perhaps in some you will find good *lecturing* — which is also to be studied. Perhaps you must go to France for that. The French '*expose*' best.

. . . We were *so* amused with your account of Solms' lecture on Algae. If you stay at Strassburg, you had best be a *Privat-docent* in *Algae*. It would tell!

Farlow replied gratefully on March 24:

Dear Sir;

Yours of Feb. 24 was received a few days ago and I was even more glad than usual to hear from you. Of course, there are times when I feel anxious to know what I am to do when I get back and that anxiety does not help my studying for the present moment, but, as long as I feel that there is some one who has an eye to my interests, I do not feel so much inclined to hurry up my studies here. I cannot express my obligations for the expressions of interest in me contained in your last letter.

There are evidently two ways of studying the botany. The first, to catch the plan of work in different places and then myself to transfer that plan to America. With such an object I feel as though I had already had enough of Strasburg. I know De Bary's method of investigation and could apply it at home — if I had time. A term with Nägeli and a short time with Hofmeister and two or three months in France would teach me the methods of study adopted by European botanists and in a year and a half I might come home, not so very badly prepared, at least, to tell what other people are doing.

The second plan involves in addition a continuous work under the different professors and requires, I am afraid, double the time required by the first mentioned course. In pursuance of this plan, the most thorough and the one I should rather adopt, if I could spare time, I ought to stay longer in Strasburg. Accordingly, when I left Str. a week ago I told De Bary that I should probably return for the months of May and June. I left my preparations and microscopic affairs there. Next winter as I have said, I expect to go to Munich . . .

If you were amused at the account of Solms lectures on the algae you would also have been amused at those on the lower fungi. He told us that we must regard the Myxomycetes as animals for he didn't have time to lecture on them. If he had had two more lectures he should have made them out to be plants. He tumbles over his chair puts his fingers in his mouth and goes through contortions that would make a cat laugh. He is goodnatured however. Just before

I left Strasburg I invited all the botanists, eight in number, to dinner. De Bary was at one end of the table and I at the other and Graf Solms in the middle where he kept up his old habit of swigging down red wine. Suppane[t]z the Austrian was more blooming than ever and went into rhapsodies over some figs on the table, Süd-europäische früchte, Liebe[r] Gott, classische, colossale, ganz eminente. He flourished his knife in a way to make me fear for my life. The supper lasted, for Strasburg, a short time, that is from half-past eight till a little before twelve. The greater part of the company then retired to the Deutscher Kaiser to finish up with a few glasses (10 or 12) of beer. The laboratory broke up last week. Suppane[t]z is ordered by the Austrian government to Prague instead of Gratz where he will probably get a place; Rostafinsky went home, Döl-brach to Bonn and I cleared for Frankfurt. At Carlsruhe I ran into Graf Solms who was characteristically trying to back round a corner. He spent the time till we reached Heidelberg in repeating to me how 'komisch' it was that we had met . . .

You are getting so modest in your demands that in your next I expect to hear that I must study the ferns, as, in course of time, Eaton will depart this life, and possibly the flowering plants also. The lichens I should like to study and in Geneva where I understand one can learn French better than in Paris. However, I must first learn German, and it will be provoking to find that just as I am getting so as to be comfortably up in the language I must go home.

Rostafinski's plans of studying algae with Thuret raised the hopes of Farlow that he might be as fortunate, for, as he said "There is no one in Europe I should so much like to study with as Thuret." On receiving the letters of introduction which Gray wrote to Thuret and Müller, Farlow lost no time in sending them off to the two men, with his own request for permission to study with them. On May 12, Farlow wrote:

I had a very polite letter from Thuret saying that he should soon leave Antibes for the summer and he should be happy to have me meet him this summer in the vicinity of Nantes or next winter in Antibes. I accepted the latter alternative with pleasure . . . Thuret wished, in very polite language, to be remembered to you and Mrs. Gray.

Of the new semester's work he said in the same letter:

I omitted to write you, I believe, that, during the vacation, De Bary had a call to Vienna which he declined. Since then Sachs has also declined. It appears that the University in Vienna is on the decline in every department except medicine and there is so much intrigue and chicanery at work there that no distinguished men will accept positions. It was a disappointment to me that De Bary didn't accept, as I should have then spent next winter with him in V. and this summer have gone to Munich.

. . . I returned here at the beginning of the semester. I do not regret returning. In a social point of view, the laboratory is not so pleasant as last semester since Rostafinsky and Suppanetz are gone and Stahl is at home most of the time getting ready for his examination in June. A new Pole is here, a beginner, and three students who only work three afternoons in the week. But De Bary is capital. He has a general course from 8 to 9 which I don't hear and then is in the laboratory nearly all the morning. He pays me a great deal of attention. At

present, I am on the Uredineen and I am cultivating rusts and inoculating sound barberry leaves. I have an ergot Cultur underway but, I am afraid, the material is poor. Tomorrow I begin on Cystopus. There yet remain the fermentation pilze which I shall soon take up. We have all been catching flies for Saprolegnia and Achlya. De Bary began today on Medicinal Plants to a class of 36, a large number. It was simple and practical, in short, excellent. The same cannot be said of Graf Solms' lectures which I attend simply from personal attachment not because I like the lectures. Systematic Phaenerogams is a hurried humble-jumble and Parasitic Phaenerogams once a week, considering the materials which he has spent so much time and money in collecting, are not very interesting. He is soon going to publish something on the latter subject. He has several 'junks' of Rafflesiae in alcohol . . .

The semester opened a week ago, on Thursday. De Bary, as Rektor, gave an address, delivery abominable, substance very good. De Bary's personal appearance is certainly unprepossessing. Everyone says, at first, is that man really De Bary? In the evening was a torchlight procession in honor of the three professors who declined calls to Vienna. The scene was very interesting as the procession passed by the Cathedral to the Palace where the Professors addressed the students. Afterwards they all marched to the Kleber Platz and burnt up their torches in a heap and sang Gaudeamus as at the funeral of a Chor student. At ten o'clock began the Commers and lasted till nearly morning. I left at two. *4500* glasses of beer were drunk on this intellectual occasion. The singing was at first splendid, afterwards slightly confused. There were generals and professors and students all mixed up together drinking, all dressed as gayly as possible. Gilkinet and I sat in the gallery where we could see the whole performance. There were speeches, songs, Salamanders, shouts, howls, and above all drinking. The scene was new and interesting and I shall not forget it. Several times the waiter upset our table with several glasses of beer upon it. Fortunately, the beer ran over onto Gilkinet's side and he soon got to be tolerably wet. After a while I found myself about ankle deep in beer and concluded to find another seat. I am happy to say that I managed to get along without what is here known as a Katzenjammer or as the students say a Kater. In the course of the evening Graf Solms appeared miraculously in the gallery with Dr. Schmitz and we drank the health of the University. What that means I don't know. Afterwards the ubiquitous Graf wandered round the hall holding up an empty beer glass until after a while he lighted upon General Carminsky and the two talked 'Faderland' till morning.

A letter from Cambridge in June left no doubt that Gray was looking forward to seeing Farlow established at Harvard on his return to America. Gray also suggested that it would be well to begin to work up some special research which could be published within the coming year, and Farlow immediately responded to the idea.

June 9, 1873

My Dear Farlow

You know how very busy I must have been — and still am, with Goodale gone since May 1. So you see why I have not promptly responded to yours of March 24, from Frankfurt, April 23, Heidelberg, & May 12, Strasburg — And even now — late in evening, I can only write a line.

You seem to be going on famously well, and doing your best and wisest. I am glad you are back with De Bary. And I like the way you are laying out your work for summer and winter. Especially I am glad of Thurets kind response to you, & that you will get some time with him next winter or early spring . . .

Mrs. Curtis writes that she has sent the Fungi; but I have not yet heard if your Father has received them.

Goodale has been appointed '*Assistant Professor* of Vegetable Physiology & Instructor in Botany.' — his name will go to Overseers, I think to-morrow. The Corporation begin to be anxious about Bussey School — wants some Professors to work there — & are *determined to have students*. Things are shaping as fast and formally as I could wish, — and when you come home — unless you are called away by better offers elsewhere, you will certainly find something to do here. I see they are beginning to *want you*, and you may fairly count upon offers when you come home — very likely before. I have said, that you can't think of returning to America for a year yet — perhaps not even then — your time & opportunity in Germany too valuable.

I think it best for you — after consulting De Bary as to what — to get up some research which you can publish in Germany — or — still better — send over for me to publish here — in Amer. Academy — or in Amer. Naturalist — according to circumstances.

Then another thing will be very well.

Send home to me, from time to time, notices of interesting works or researches, Botanical news — criticisms &c — to be published with your initials among the miscellanies in Sill. Journal. Or, if you will write some sprightly letters on such matters that I can print in Amer. Naturalist, it will be well.

But lose no working time. Solid work is better than any display . . . All good things you can send me to print, under your own name, or initials — which will duly be known — will be advantageous . . .

Ever yours truly

Asa Gray

Strassburg, June 30.

Dear Sir;

Shortly after sending my last letter I received your letter of the 9th and the notices of the death of Dr. Torrey and Sullivant. I was quite surprised to hear that the latter was dead and I had no idea that he was so old a man . . .

I have spoken to De Bary about some special work. He proposed, The fertilization and fructification of Agarics, ditto of the Oscillariae, ditto of Scenedesmus and the Entwickelung (cultivation and growth) of Oidium lactis. The last I selected but, as one may work an indefinite length of time without meeting with any success or result, I have also taken as nebensache or collateral, the microscopic structure and growth of the Haftorganen i.e. the disks of Ampelopsis. and the rootlets of Tecoma &c. From one or the other I ought to make out something.

As for writing notices, I should be glad to do that and must try and find time in the vacations. The following subjects suggest themselves: late German Botanical Text Books, reviewing Sachs and Thomé, Rostafinsky's Myxomyceten; or, possibly, Cornu, Saprolegnieen and Strasburgers, Coniferen. For light letters, an account of Upsala, and botanizing in the Vosges. I might give a detailed account of the management of De Bary's laboratory and cultures but that is too much like letting the public into the secret . . .

I should like, after finishing my studies to write a compendium, something like Sachs, about Algae and Pilze, giving the latest modes of working and results so far as known. Such a book is very much needed as Berkeley is thoroughly bad and there is positively no other book in English. The writing would serve for me as a review of my studies and could also be available as a text book in classes where I had to teach the subject. The first thing, however, is for me to learn the subject myself. Remember me kindly to Mrs. Gray.

Yours, respectfully, but tired,
W. G. Farlow.

Farlow left in August to spend a pleasant six weeks in Geneva with Dr. Müller, and returned regretfully to Strasbourg in October, to take the last of the courses with De Bary which he needed to round out his studies there.

Strassburg, Oct. 27th.

Dear Sir;

I am again installed in Strassburg and, after Geneva, dislike the place more than ever . . . In comparison with other cities, it seems barbarous . . . I found my six weeks in Geneva very profitable. Dr. Müller gave me an unlimited amount of attention and I was surprised to find how much, with his help, I was able to do in so short a time. I have no means at present, to repay his kindness. Of course, I thanked him and gave him a book as a memento, but it would give me pleasure, if you should hereafter, if you have occasion to write to him, mention that I have written you that I feel under obligations to him for his attention and instruction. While in Geneva, I had an opportunity to make several excursions into the Jura and mountains near the city, in which excursions I succeeded in collecting a large number of lichens which are not only numerous but valuable, as I have studied them tolerably carefully, and, also, saved duplicates with an eye to future exchanges in America. Although, perhaps, having studied lichens under the most favorable and agreeable circumstances possible, I must confess that I think they are the stupidest and, at the same time, the most difficult plants I have ever studied. However, if it is necessary for me to study them in America, I feel as though I had had a good preparation here. During the summer, I find that I have collected quite a lot of Erysipheen, Uredineen, Perinosporeen, and the smaller fungi generally and have laid by a number of duplicates which I think will be of good service, hereafter, in America.

So far, although I have constantly regretted being obliged to shut myself up in Strassburg, I feel as though I could no where else and in no other manner have learnt so much of that part of botany which I needed to know and which is in America entirely unknown, certainly practically. I have now learnt so much anatomy, that is histology, that De Bary doesn't advise my spending any more time on it here. Then I have studied the orders of minute fungi tolerably thoroughly. Of course, to know the subject perfectly is a work of a life time. I have learned De Bary's method and, although a knowledge of forms and species is here discouraged, I have learnt a great many forms and, with the knowledge of their development and growth which I now possess, the determination of species comes tolerably easy . . .

The remaining botanical subjects which I wish to study are, the development of the higher cryptogams, i.e. the fertilization of ferns, Marsilea, &c; second, the algae, marine and fresh water in respect to their development, and physiology;

and thirdly, a short study of the cell, in the abstract, as has been done by Hofmeister in his latest work. Whatever I now study comes a good deal easier than it did a year ago, showing that the somewhat slow preparation recommended by De Bary is beginning to bear good results. I don't feel as though I wanted to stay much longer with De Bary. I should only be continuing the same work I have already begun. It is simply now a matter of detail and, if I am to stay here, till I know all he can teach I shan't get back to America for a long time. If I should stay here too long, I should be more likely to turn out a botanist of the Graf Solms type rather than like De Bary himself . . .

If I can return in September, as I hope, the question will be whether to go to Pringsheim in May, which will perhaps be decided in the negative if I study fresh water algae with Thuret, which may be possible, or to go to Würzburg where Stahl and Gilkinet are going, and study the cell under Sachs and attend his course on general Vegetable Physiology without actually going through the experiments in his laboratory. In this way, I should get all the Physiology necessary for me as a cryptogamist. Brefeld is also a physician in Würzburg and is now studying hefe pilze and, perhaps, could help me some. I hardly think I should find it profitable to stay more than two months in Würzburg. Afterwards, there is nothing which I care specially to do but, perhaps, to visit Rabenhorst, of whom both De Bary and Prof. E. Fries have a poor opinion. Then a few days on the coast of France or England and a visit to the leading English cryptogamists . . .

I think now I have stated the whole story. I should like to return next fall. If it is necessary for me to remain a year longer, I must simply spin out longer the list of studies I have mentioned and, possibly spend a short time with Strasburger in Jena, and, although I should at the end of that time know more botany, I should, perhaps, be less qualified to teach in America. Whether, as far as America is concerned, the opportunity is good for my returning you know very much better than I. I think you also know from a former letter of mine that, although I should like to return to Cambridge, I don't feel as though my future success entirely depends on it. As I then wrote, I should like also to go to either New York or California should there be opportunity in either place. At any rate, I feel perfectly safe in trusting to you, knowing that you will recognize what is for my best interest.

Here in the laboratory are Stahl, Gilkinet, and Carminski who remain, and two new students, Prof. Czerokin of Kasan, Russia, and Dr. Linsted of Berlin who has written on Saprolegnieen. After Christmas, Dr. Schmitz goes to Halle as Docent and Rostafinsky, who is now in Petersburg, may take his place. De Bary repeats his course on Anatomy and soon expects to finish his book. Graf Solms also repeats his Cryptogams. In accordance with your request I have written four gossipy letters for the Naturalist. I do not know whether you will not find them too light . . . I also send the promised critique on Thomé and Sachs. In a few days, I shall send notices of some recent magazine articles. I hope to be able to send you soon a critique or summary of the Lichen, algo-fungus question . . .

Farlow

These "gossipy letters" were edited by Gray, and soon appeared in three parts in the American Naturalist, vol. 8, 1874, under the title *Notes from the Journal of a Botanist in Europe. Part I. Sweden. Part II. Norway, etc. Part III. Geneva and the Alps.* Gray wrote on November 17:

I am arranging for the articles in Am. Naturalist — to begin in Jany no. Naturally enough these articles are not so *sprightly* as your letters. I wish you would write only on one side of your paper. I should like to *season up* the articles by *sandwiching* in bits from your letters.

On January 11, 1874, he announced their publication:

The 1st part of your *Notes* is in Jany Naturalist. It reads nicely. I have had proofs of the 2d — for Feb. number. I made it up out of your letters!

In the meantime, at the suggestion of De Bary, Farlow was at work on a new topic for research, which was to culminate in the much discussed paper on the discovery of apogamy in ferns. He first mentioned it on December 7:

Last week was rather an eventful one. I discovered an entirely new Puccinia on the common mallows in the botanical garden which germinates at this season. I presume De Bary will cultivate and describe it. My work has been the fertilization of ferns and a week ago De Bary found a case in Asplenium molle where the fern is produced directly from the prothallus without the intervention of an archegonium and fertilization. He gave that to me, as a subject to work up and I am now working busily away and have made quite a number of drawings. I shall finish the work as soon as possible for I want to go to Paris with Jackson about Christmas. However, I cannot leave till my work is completely done. It will probably be printed in the Botanische Zeitung and I will send you the same in English for the Am. Journ. if you wish. What shall be done about the figures? Had they better be copied from the Botanische Zeitung or shall I send you the drawings?

With regard to the letters I sent, it was difficult to say just how 'sprightly' I ought to make them. What I sent would here have been considered dreadfully frivolous. You did not say whether they wanted any more or not. You can make any insertions you please, but I don't want the editors of the Nat. to change anything.

With regard to sending criticisms or abstracts of magazine articles, now I am too busy with my paper to write any and if I am to leave sooner than I expected for home I ought to spend my whole time in study . . .

You will of course imagine that I am anxious to know what will be done in Cambridge this summer as my plans and travels may be so much affected thereby. If the college determines to introduce the study of cryptogamic botany I hope they wont forget that they must have microscopes. I think you will be disappointed when you again see me in the amount of systematic fungology which I have learnt but, on the other hand, I have studied development as applied to some of the minute forms, to a much greater extent than I should have ever done if I had not come abroad.

The work on the fern prothallus was not finished by Christmas, so instead of going to Paris, Farlow joined his laboratory friends Rostafinski and Gilkinet, in celebrating Christmas Eve with the lively De Bary family. On the following day he wrote his mother and sister his description of the festivities.

Strassburg, Dec. 25.

Dear Mother:

I take advantage of today to wish you all a merry Christmas. As far as I am concerned the day is Sunday intensified, that is very much quieter than Sunday is here. Last night we were invited to De Bary's to see his Christmas tree. It was all explained to me as though I had never seen a tree and Rostafinsky with his usual impudence and fondness for giving good advice hoped that I would introduce the custom into America. After the tree we had supper and at the end I was presented with a plate of candy on the top of which was a mock sausage for my benefit as I am so fond of such things. Today the weather is raw and foggy and I gave up my project of a long walk thinking the less I got of such air the better. This morning Rostafinsky made me a long call and this afternoon I called on Dr. Alexander and also took a look at the Vespers in the Cathedral where the altar was all lighted up. The soldiers had a tree in St. Thomas church but as there are 18000 soldiers I imagine that if one tree held all the presents a good many must have gone without. I should have liked to have seen the tree as it stood directly in front of the celebrated monument to Marshal Saxe which is very large and of white marble. Today the city has swarmed with soldiers and many of them being new recruits the uniforms looked clean and fresh.

Last Saturday I went to Frankfurt to see the Websters . . . In the evening a woman called who is keeping an American boarding house in Frankfurt. She had her pockets filled with great red apples she had received from America. After being so long in Strassburg the Frankfurters looked very swelly. The greatest treat at the Webster's is an American bed. That is, they don't have the feather plumons which the Germans always have. The plumon is a thing too large for a pillow and not large enough to cover me although I am not very long. Consequently as they don't have such things as bedquilts and comforters one's feet and hands are always out in the cold . . .

Your affectionate son,
William

Strassburg, Dec. 25.

Dear Mary:

Unless you consider yourself included in 'our' family I will wish you and your family a Merry Christmas. Last night I went to De Bary's to see his tree. I wasn't there at the opening at six o'clock being too busy to go so early. Mrs. Gilkinet was there and said there was a great running about and trying to look through the key hole. When I arrived I found the Norway spruce already lighted up. Instead of the hideous wax doll called the Christ child there was a bunch of gilded pine cones which Mrs. De Bary had prepared for the occasion. There were four children Wilhelm the oldest who was very sheepish and didn't say a word, August more lively, Marie, a rather too lively young lady six years old with a pug nose and little black eyes which are generally half strained out of her head by the two black pig tails on the back of her head, last night however her hair was flying in all directions, and little Hermann with very red cheeks and blue eyes. The presents were put on little tables near the tree. Hermann was perfectly absorbed and could not say a word all the evening. All the boys had a lot of soldiers. They seemed to have more toys than American children. Marie had only four dolls and a doll house besides a lot of other stuff and the older ones had books and games. The candies and cakes were a sight to behold and

came from all parts of Germany from Berlin to Strassburg. There was a young law student named Von Jacoby from Berlin who had a table with presents. He seemed to be a sort of protégé of Prof. De Bary's. He had an enormous mouth out of which he spilt his words. His conversation was principally about how much beer they could drink in his Chor. On his table were a lot of gloves, and such gloves. The Germans never wear any which are not ten sizes too large. There was an indescribable thing which Rostafinsky told me was a Pommern Gänsebrust and a great delicacy. It was a great goose skin sewed up at both ends and dripping with oil. It was full of uncooked smoked goosemeat and said to be delicious! The Pommern geese are very celebrated. There were also Erlangen Lebkuchen and Berlin gingerbread and two cans of condensed milk on his table. Mrs. De Bary had amongst other things two bottles of arrack punch. At the table the company appeared much interested in my description of peanuts which they thought couldn't be very nice and they couldn't understand how our You meant the same as their Du Ihr and Sie. They of course didn't see the point when I explained how senseless and inconvenient their Sie was. The other day De Bary had a great present from America. A box containing a swamp pitcher plant, some hickory nuts and twelve cranberries. As I said they were good to eat they cooked them and liked them very well.

Today I dined with Gilkinet. I asked if his little Georgie 11½ months old was coming home with his Alsatian nurse to spend Christmas. Ever since he was born he has been farmed out to a peasant near Strassburg. His mother didn't know whether he would be at home or not today but had an idea, she wasn't quite certain, that he was to spend the day in some village near the Vosges. Such independence in a child of his age would be remarkable in America. His mother seemed very calm and philosophical on the subject. So is Prof. De Bary who after giving Hermann a glass of wine before sending him to bed refused to let August, aged 8 or 7 yrs., have any more because he brought on a typhoid fever last year by drinking too much punch New Years.

Will.

A few more days of hard work brought the paper to its conclusion and on New Year's Day, 1874, Farlow wrote:

Today I send you the manuscript of an article on which I have been studying some weeks. I presume you will think it very short for the time spent upon it but all such work requires an everlasting amount of time. The article seems to me not at all adapted to the Naturalist but rather to Silliman's Journal. You are at liberty to dispose of it as you please provided, wherever it appears, the drawings are as good as those usually found in the Botanische Zeitung. I must make this a condition because when printed I should feel bound to distribute a few copies in this region and if the execution was not up to the mark there might be some very invidious remarks made about American publications here. I wrote the first draught in German and then afterwards wrote in English, having shown the German to De Bary. I have made a German abstract which will appear at once without plates in the Botanische Zeitung . . .

There may be a good many clerical errors in my manuscript but there is no English speaking person here to whom I can show it. I should like to see the proof of the figures I send before the article is published.

Gray acknowledged the paper with a brief note on January 11:

Your New Year letter, and the Mss. & drawings have come to hand. I write a hasty line to tell you so. I think of reading your paper to the Amer. Acad. And, if they will engrave the drawings to print in the Proceed. — otherwise in Naturalist unless Sill. Jour. will engrave on stone or copper.

We will see soon. Your paper is very interesting!

On February 15 Gray sent the first proof.

I read your paper to Amer. Academy — it is accepted for *Proceedings*. Here is a rough, unread proof. I may keep it back till I can get corrections from you.

That the figures may be photographed for the new process of reproduction, I have to get the drawings gone over in *pen & india ink*: pencil will not do.

Farlow returned the proof from Antibes, where he was enjoying at last his long anticipated visit with Thuret, and he protested that the drawings were not in pencil, but in neutral tint, and should not be inked over without his supervision. On March 21 he wrote again from Antibes.

A week ago I returned the proof corrected which you were so kind as to send me. Last night Rostafinsky and Janchewsky arrived and Rostafinsky said that the fern prothalli are now grown up into Pteris cretica instead of Aspidium molle. Today De Bary sent me a copy of the Botanische Zeitung with my short German extract in which he had changed Aspidium molle to Pteris cretica throughout. Will you be kind enough to do the same with the English or if it is too late for that, to write a little note at the end saying that in consequence of the further development of the prothalli enabling the species to be accurately determined it is necessary to change the Aspidium for Pteris cretica throughout. I thought myself that it was more probably Pteris cretica but De Bary thought the contrary. Now it is settled.

Gray replied disconcertingly that the "proof returned came *just too late*," and Farlow wrote again on May 4:

I reached Strassburg Saturday evening and, on my arrival, I found yours of Apr. 10th which had been forwarded from Antibes. I am quite anxious about the publication of my article as the preliminary remarks in the Botanische Zeitung have been attacked on all sides particularly in Berlin and Würzburg. I don't understand exactly what you mean by my proof arriving too late. I hope you do not mean that it has been published just as it was. If so, don't let a copy get into Germany for there were some horried mistakes in the quotation from Wigand. I hope also that the drawings are well done otherwise the remarks of the Germans, who don't believe the thing because it is contrary to their theories. will be disparaging to the last degree. For De Bary's sake as well as my own I must be extremely careful about the form in which the article appears.

The paper, titled *An Asexual Growth from the Prothallus of Pteris serrulata*, was published in the Proceedings of the American Academy, vol. 9, 1874, and the correction of the name to *Pteris cretica* had to be made in a note at the end. That Farlow was far from satisfied with the appearance of the article is evidenced in his letter of May 21 when he wrote:

As to the Fern Prothallus, I must confess that I was decidedly disappointed in the appearance of the plates which fall very far short of those in the Bot. Zeit.

and the criticisms have been so unfavorable on the execution of the plates that I hope you will not distribute any copies in Germany. I may have to translate and reprint the article with better plates in the Bot. Zeit. As it is, I shall have to leave any preparations, which I should like to take to America, with De Bary who will show them to other botanists. I hope you have kept the drawings . . .

As Farlow's sojourn in Europe drew near its close he became more anxious about what he was to do on his return to America, and although Gray hoped and expected to have a place for him at Harvard, there was as yet no official confirmation of a position. On March 27 Gray wrote:

Presdt Eliot is in England, till May . . . My *impression* is that he wished in spring to put you on to the Bussey-foundation, & do some work there — perhaps start a *fungus*-laboratory — as well as here. I'll be responsible that you get the full of an Assistant-Prof. salary . . . & in time more . . .

As the Bussey develops, I think it likely summer work may be in order there. But if so, you will have an equivalent in winter. Do not fear about that.

The President is getting a great idea of the importance of *research* — and will see that you have a chance. As to Goodale's course of this summer, — as you need not begin till *1st August*, I advise you to agree to come & take it then — give a month to it — if you intend to come home this fall at any rate. — If you think of staying another winter, that is another matter.

I think it will be for your advantage to take hold of this work next summer — tho' it wont amount to much. It makes the right beginning.

The last letter from Gray which we have of this long correspondence expressed once more his confidence in the establishment of a cryptogamic laboratory.

Cambridge May 4, 1874

My dear Farlow.

Yours of April 11, came while I was in Washington. What I & Goodale had previously written will by this time tell you all we can say. Presdt E. is not home yet, — but may be expected to-morrow.

I see you are decided to come home this summer, any how. In that case, I would, if I were you, get home before the end of July. But I would not hurry to be here sooner. The summer course this year will not probably amount to much, but will lay a foundation. The plan of Goodale taking it for July, & turning it over to you in August is best — You will then work independently. And you can go on into Sept. if you like. We shall, I doubt not be able to establish a laboratory for Cryptogamic & Anatomical Botany, in which you can make name & fame . . . Your paper has done you great service. More of them, and your success as teacher will do the rest . . .

I am sadly hurried — will write again soon.

Ever yours cordially
Asa Gray

Farlow sailed for home in June, 1874, and immediately received from Harvard his appointment as Assistant Professor of Botany. He entered at once upon the task of creating a laboratory in cryptogamic botany and

phytopathology, and his subsequent success at Bussey and later at Cambridge has fully justified the years of preparation that had gone into his European study. He continued to keep in contact with his friends in Europe, and his correspondence with De Bary was maintained until the latter's death. A letter written by De Bary in the summer of 1877 is of special interest, as by giving news of the whereabouts of the students in the laboratory, and of the completion of De Bary's own book, it brings to a natural close the narrative of Farlow's two years at Strasbourg.

Montreux, August 17, 1877

Dear Mr. Farlow:

From the stamp you will see that I have taken refuge in order to get peace, not in the desert like the ancient prophets, whom I do not emulate in general, but at that very corner on Lake Leman which you will remember as beautiful and quiet, especially at this time of year, because now people think it is too hot, while in winter the English and Berliners abound here. When you imagine this charming bit of land and water where redwood and giant trees, together with laurel, Laurocerasus, Punica, and Passiflora are being reflected in the blue surface of the lake, and right now the whole atmosphere is filled with the scent of the flowers of Ligustrum japonicum — while nearby the Phyllachora Congress is having its international meetings, and the wine ripens untouched by the disease these gentlemen are talking of — if you imagine all this, your heart will probably become reminiscent too, and you will forgive an old sinner inasmuch as he has retired to this idyllic spot in order to write you at last.

The narrator may proceed subjectively, so, I start with the news that the Anatomy is entirely finished four to six weeks ago, 640 plus 20 pages strong. I have been working desperately on it since last summer, except for a short interruption which I shall mention later. That's why you did not hear from me for so long a time. Two proofs which may have surprised you in April have been sent to you as a mute sign of life on my part . . . Since the middle of July I have finished everything excepting the last proofs. I have become human again, but I had to arrange and clean up so many things, and consider so many other matters that I could not find time to write a detailed letter.

The cleaning-up-process also includes working up the material that has collected in the last three or four years for your 'Pteris story.' I have finished this during these last days, and I hope to have cleared up this matter now as far as three species of ferns are concerned. Your side is thoroughly correct. It turns out — when compared exactly — to be a very instructive special case of a common phenomenon. With this I have come to answering your letters, for yours of last October starts with Pteris. As for the complaints which you add to your remarks, I can answer only with the wish that you may not take to heart what the American botanophiles and mycophiles say about you and Cooke. If you continue in the same way as in the last papers you sent me, these gentlemen will certainly come to reason, if not all, then at least many of them. It's not different with me: Berkeley and his set always consider my kind as very wretched fellows, and still, by now, all intelligent people know what they should think of it. The only way to achieve something, is not to let yourself be bewildered. I had a lot more trouble with Hallier, Sollman, Hoffman etc. than anybody has nowadays.

To continue with your mycological questions — I see in Fischer von Waldheim's new survey of the Ustilaginales that he indicates Urocystis colchici on

several monocotyledons, also on Allium (A. rotundum). On another Allium, A. magicum, he indicates another Urocystis species, U. magica. This is about all that is known about Urocystis on Allium. Considering the great similarity of the spore characters of most of the species a decision on the identity of species seems to be very difficult to me. Why are not all species of Urocystis, or all species on monocotyledons, a single species? I shall send your fungus to Fischer, so he can compare it with his materials. But I shall wait till winter because he is probably absent from Warsaw now . . .

You know all that's important about Rostafinski, that is that he is in Krakov, very industrious, and with good success. I would not know much more myself without going into details. As for Stahl, you probably don't know that he is privatdocent in Würzburg. He went there in the summer, and is very satisfied with everything thus far. I talked with him recently in Strasbourg, and I hope he will also write me.

You do not have to fear that Gilkinet has forgotten you even if he should not have written to you in the meantime. I can tell you so with certainty, for we talked about you very much when I visited him last April in Lüttich. Here in Strasbourg we had the same experience with him as you had — not a word from him. In April I had a chance to go to the Horticulture Exposition in Amsterdam, and I took advantage of this trip to visit him in Lüttich. After a very friendly reception, I soon found out the good reason for his personal and botanical silence — he has accepted the position of chargé de cours at the University, that is to say for pharmaceutical chemistry. He has to manage a chemical laboratory and, at least for a long time, he was entirely absorbed by the necessity of remodeling himself for work in this new job. This is understandable and it will justify him also in regard to you . . .

There is not much to say about Solms except that he is as you know him — short active semesters, alternating with long vacation trips. Last fall he was in Moscow and Stockholm, at Easter in Athens. Otherwise there were many changes in this laboratory. Dr. Wilhelm, whom you knew, is the only stable man. This summer it was very lonely, since, because of illness and other reasons, only two people were working. I shall send you some dissertations which were made during the last year . . .

In order to achieve a symmetrical conclusion to this letter, I shall talk subjectively again. In my house, everyone is rather well, and so am I, especially the last four to six weeks. I accompanied my family to Switzerland yesterday — they are in Interlaken again. I left them for a week to go here, and then to Bex (Rhône Valley) to the Swiss Meeting of Natural Scientists, and then to Interlaken, too. In mid-September we shall be at home again.

Goodby for this time, best regards from my family and myself, and do not punish me by too long a silence.

Sincerely yours
A. De Bary

HARVARD UNIVERSITY
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NOTES ON FARLOW'S AGARICALES FROM CHOCORUA

Rolf Singer [1]

To those who are acquainted with the White Mountains of New Hampshire, the name Chocorua brings to mind the beautiful lake in which is reflected picturesque Mount Chocorua, but few realize that the region is a mycologist's paradise. In this region of beeches, birches, maples, oaks, pines and hemlock at the lower elevations and of spruce and balsam below the nearly bald granitic summit of the mountain, there are habitats of widely varied types and ecological conditions that make possible the occurrence of many different species of fungi.

Whether it was beauty of the scenery, the presence of congenial faculty colleagues, or the presence of these varied and excellent collecting grounds or some other factor that caused Dr. Farlow to choose Chocorua as his summer home it is impossible to say. However, it is obvious from the large number of fungi in his herbarium which were collected in this region, that when he left his office on the top floor of the Botanical Museum in Cambridge where his herbarium was at that time maintained, he lost few opportunities for going out into the woods of Chocorua and vicinity to make collections of fungi, many of which he sent to his colleagues who were interested in special groups. Among these colleagues was his friend and former student, Dr. E. A. Burt, who cites many of Farlow's Chocorua specimens in his monographic studies of the Thelephoraceae and who in 1909 wrote to Farlow that he was turning up many unusual and interesting species of *Hypochnus* (i.e. *Tomentella*) in that region. While he was making these miscellaneous collections, he also had in mind the eventual publication of an illustrated agaric or mushroom flora and it was at Chocorua that Joseph Bridgham and L. C. C. Krieger, under his critical supervision, executed in water color and at natural size a large number of exquisite illustrations, a small number of which have now appeared in the "Icones Farlowianae." The larger proportion of the paintings that remain unpublished, and unfortunately not available except to those mycologists who come to the Farlow Herbarium to study specimens and plates, nevertheless are extremely valuable as permanent records of the fungi as they appeared when freshly collected. They are also of value because they illustrate species which, although Farlow collected and determined many of them, were in many instances seen and verified by Charles Peck of the State Museum at Albany, the foremost American agaricologist of the period, as well as by European specialists in the group. Thus Chocorua was the source of a large number of specimens, not only for Dr. Farlow but to many colleagues with whom they were shared, as well as for those who

[1] The writer is indebted to Dr. David H. Linder for the introduction to this article.

were fortunate enough to be guests at his summer home and do their own collecting. However, despite the sharing of specimens and his efforts to have identified accurately all those fungi that were illustrated by his artists, there nevertheless remain many that are still to be identified. Also, there are others which, although named as accurately as the knowledge of the time permitted, are worthy of additional notes. A few of these are discussed in the following paragraphs.

I. SUILLUS GRANULATUS IN NORTH AMERICA AND EUROPE

One of the common species observed near Chocorua is a bolete generally determined as *Boletus granulatus* L., and under this name illustrated and described in the *Icones Farlowianae*(3). This species, described by Linnaeus and Fries from Sweden, has previously been studied by the author in Europe. It was, therefore, interesting for him to compare his notes with data gathered on abundant living material at Chocorua. We shall at this point anticipate our eventual conclusion, and stating that the American form actually differs macroscopically, microscopically, and ecologically from the European type, name it **Suillus** [2] **granulatus** ssp. **Snellii** [3] Sing. ssp. nov.

Suillus granulatus ssp. **Snellii** ssp. nov.

Notis macroscopicis subspeciem *leptopodem* fortiter revocantibus in mentem; notis chemicis haud notabilibus in comparatione cum typo; sporis minoribus (6.8–8.2 x 3 μ) a subspeciebus ceteris recedens; habitatio: cum *Pinibus strobis* (rarissime cum aliis coniferis) ad terram plerumque arenosam gregatim in America boreali; collectio typica in Farlow Herbarium conservata atque a D. H. Linder & R. Singer prope Chocorua, N. H. collecta est.

Macroscopical characters: There is no need of wasting space by redescribing *S. granulatus* ssp. *Snellii* macroscopically. The reader will find good descriptions of it in most American books and papers dealing with boletes, e.g. in "Icones Farlowianae," Atkinson, and others. We may only add that the spore print is "Isabella color." [4]

Microscopical characters: Spores pale yellowish or pale melleous, thin-walled ellipsoid-subfusiform, frequently attenuate toward the upper end and usually with a subhilar depression, 6.8–8.2 x 3–(3.5) μ; *basidia* clavate, 4-spored, hyaline, but at some spots covered with brownish resinous masses emanating from the cystidia, 20–24 x 6–7 μ; *cystidia* frequent, extremely crowded on the pores where their excretions or incrustations cover large portions of the hymenium, honey color or hyaline when seen without the incrustation (after crushing the preparation in an alkaline

[2] Since it has been proved by D. P. Rogers that S. F. Gray's "Natural Arrangement" is post-Friesian, the author thinks that Snell's (12) proposal of the name *Suillus* instead of *Ixocomus* is justified.

[3] W. H. Snell was the first accurately and reliably to indicate one of the main characters which separate the American subspecies, i.e. the small spore size; we therefore feel that it is most appropriate to dedicate this subspecies to him.

[4] All colors cited in quotation marks are those of Ridgway unless indicated otherwise.

medium), thin-walled, smooth, clavate-fusoid, more rarely claviform or fusiform, or ampullaceous, or subcapitate, or a combination of these, 34–38 x 6–10.5 μ; *trama* of the tube walls divergent and with a distinct regular mediostratum; *cuticle* with a distinct ixotrichoderm, some of the hyphae somewhat incrusted; *hyphae* of the context hyaline, thin-walled, consistently without clamp connections; *glands* consisting of masses of strongly incrusted and pigmented or rarely hyaline dermatocystidia similar to the ones observed on the pore mouths, but more variable in size.

Chemical characters: **KOH** on the surface of the pileus: lilac, becoming grayish brown in a few seconds, young stages more intensely and persistently lilac; on the context of the pileus: lilac, becoming grayish brown, young stages usually not showing any reaction except where the context is showing the first yellowish spots (near the tubes); on the pores and tubes: almost black immediately. — **NH_3** (prolonged action) and **NH_4OH** on the surface of the pileus: slowly lilaceous brown, in young stages slowly gray or grayish brown; on the context of the pileus: lilac (except for young stages with no appreciable reaction); on the pores and tubes: lilac (except for young stages with no appreciable reaction).

Habitat: On the ground in coniferous and mixed woods, in mycorrhizal connection with *Pinus strobus* and *P. monticola*, rarely, near stands of these pines, transgressing into pine-free stands of hemlock (*Tsuga*) or fir (*Abies*). Fruiting from June to November.

Distribution: The distribution of this subspecies has not been established satisfactorily because of the fact that many species have been wrongly determined as "*Boletus granulatus*" in the past. The geographic area of ssp. *Snellii* most probably coincides with that of the five-needle pines with which it is associated.

COMPARISON WITH CLOSELY RELATED SUBSPECIES AND SPECIES OF *SUILLUS*

1. **Suillus granulatus** (L. ex Fr.) Snell ssp. **typicus.** In the American form the context, tubes and the stipe remain white or pallid for a longer period than in ssp. *typicus*; the color of the pileus and the glands of the stipe reach darker tones than in the latter. In our specimens from Chocorua as well as from Massachusetts and New York, the color of the well-pigmented pilei was about "orange cinnamon," sometimes near "cinnamon rufous," or darker than these, and then reaching almost all shades familiar in *Suillus luteus* (L. ex Fr.) S. F. Gray, but pale or bleached forms are not uncommon, the bleaching starting mostly in the center of the pileus; the glands tend to become purple or blackish long before such a change is seen in the European form. There are, however, cases where the glands remain rather inconspicuous in the American form, and others where they darken early in Europe. The former should be interpreted as transitions toward **Suillus brevipes** (Peck) Sing. comb. nov. (*Boletus brevipes* Peck) while the latter appear to be transitions toward another subspecies of *S. granulatus* which we shall discuss later. The macroscopical difference between the European type and the American form is easy to see

when good colored illustrations of both these forms are compared side by side, though the comparison of dried material does not show striking differences.

The microscopical differences between these two forms are restricted to the spore size. This, however, is the most important distinguishing character. The spores of a specimen collected by Buchholtz in the Mikhailovski Park in the Moscow region were found to be 8.3–9.8 x 3.3–3.5 μ, and other notes on ssp. *typicus* contain similar measurements which, in comparison with ssp. *Snellii*, are decidedly larger. The other microscopical characters do not seem to differ noticeably in these plants.

The chemical characters of the European form are nearly identical with the chemical characters given above for ssp. *Snellii*.

2. **Suillus granulatus** ssp. **leptopus** (Pers.) Sing. comb. nov. (*Boletus circinans* ssp. *leptopus* Pers.) (8). This is the same as the species this author indicated under the name *Ixocomus Bellinii* (Inz.) Maire.[5] Certain features of the American form, ssp. *Snellii*, seem to indicate a close relationship with ssp. *leptopus*. However, the spores of ssp. *leptopus* are not small as in ssp. *Snellii* but large as in ssp. *typicus*. The author found them 8.5–10 x 3.5–4.5 μ, and occasionally reaching 16.7 x 7 μ, and the basidia 27–29.5 x 6.8–9.5 μ in a specimen from Hammam El Cif, Tunisia, collected by Bénier and determined by Patouillard.

3. **Suillus placidus** (Bon.) Sing. comb. nov. (*Boletus placidus* Bon.). This is the same as *Boletus albus* Peck as found abundantly in Massachusetts and New York as well as in Europe by the author. Kallenbach thinks that this is the same as *Boletus Bellinii* or what we call *Suillus granulatus* ssp. *leptopus*. But this confusion merely results from the fact that Kallenbach does not know the latter except from the literature. We have found that though *S. granulatus* ssp. *Snellii* and *S. placidus* often grow associated with one single tree, side by side, there is no difficulty distinguishing these species macroscopically.

The spore size of *S. placidus* is about intermediate between *S. granulatus* ssp. *leptopus* and ssp. *Snellii*. This difference even though slight is constant. It is in accord with European measurements. Snell (11) has also indicated the slightly larger size of the spores of *S. placidus* when compared with the American form of *S. granulatus*.

While thus the microscopical difference between *S. granulatus* ssp. *leptopus* and *S. placidus* is very slight, the chemical characters which are about the same in all subspecies of *S. granulatus* including ssp. *leptopus*, tend to separate the latter from *S. placidus*. The ammonia reaction tends more towards red tones in adult specimens of *S. placidus* than in *S. granulatus sensu lato*. It is remarkable (and should be kept in mind when the usefulness of chemical reactions in boletes is discussed) how constant these reactions are if specimens of corresponding stages are compared. In the

[5] Revue de Mycologie **3**: 48. 1938. "Et si nous preferons de le classifier comme variété de ce dernier (*I. granulatus*), nous pouvons reprendre le nom de Persoon." Konrad (6) considered it as a subspecies of *S. placidus*; Gilbert (4), as an independent species.

writer's paper cited above (9), this same reddish reaction was indicated from specimens growing on *Pinus cembra* on granite soil in Europe, and later was checked on specimens growing under *Pinus strobus* on sandy soil in the Adirondack Mts. of New York, U. S. A. The results were identical. In order to give the reader the full descriptive facts as drawn from American material of *S. placidus* for comparison with *S. granulatus*, American form, we cite our notes on the chemical characters of the former species:

KOH on the surface of the pileus: lilac to violet, after a while becoming deep reddish brown to chocolate, in young stages occasionally salmoneous or purplish salmoneous at first, lilac or violet after a while; on the context of the pileus: lilac to violet, strongest on the yellowish colored places in very old specimens, and only on these places (if there are any) in younger material; on pores and tubes: reddish brown in adult specimens, but no reaction or else a slow change to pale sordid brown in young stages. **NH_3** on the surface of the pileus: lilac to violet; same reaction on the context, but none when young; on pores and tubes: *initially salmon color to reddish tan*, but no reaction when young. **NH_4OH** on surface of the pileus: stronger and deeper than with NH_3; on context of the pileus: same reaction as with NH_3; on pores and tubes: blue to gray, but no reaction on young stages; in the very old yellow stage the tubes reach "*zinc orange*" (R.) with NH_3-vapors, and the surface of the pileus becomes *brownish-red*, the context of the pileus *salmon color* under their influence.

The above data prove that Kallenbach's opinion (5) is erroneous. Kallenbach thinks that he can divide *S. placidus* into four "formae": 1. f. *Pini strobi*; 2. f. *Pini cembrae*; 3 f. *Pini halepensis* (our ssp. *Bellinii*); 4. f. *americana*. As it looks to us, if there are any forms to be distinguished, they should be f. *Pini cembrae* and f. *americana* (in Kallenbach's terminology). The European plant on *P. strobus* is either introduced with the trees, and therefore the same as f. *americana*, or it has originated from the European form on *P. cembra* (which is much less probable), and then it would be the same as f. *Pini cembrae* since there is no evidence of acquired specialization (inability to grow on the old host, *P. cembra*), nor any morphological or chemical differentiation. Kallenbach's f. *Pini halepensis* is not a form of *S. placidus* as shown above. Consequently, all he says about the geographic area of his *S. placidus* is to be understood "cum grano salis." The African and Asiatic localities given refer to *S. granulatus* ssp. *leptopus*. The European ones are almost irreparably mixed up.

MYCORRHIZAL CONNECTIONS OBSERVED IN THE RACES OF *S. GRANULATUS*

The writer has studied the typical European form in Central and Eastern Europe. It has consistently been found in the neighborhood of *Pinus silvestris*, in some cases under *Pinus montana* (in the Alps) and *Pinus uncinata* (in the Pyrenees); it has also been collected under *Pinus austriaca* in the Wiener Wald. This latter location, however, may not be the correct place to look for the typical *S. granulatus*; at least, my notes seem insufficient for definite conclusions when seen in the light of more recent developments.

The Mediterranean form, *S. granulatus* ssp. *leptopus*, was studied by the writer in large numbers on the sand dunes of the Mediterranean Coast near Prat de Llobregat, Catalonia, Spain, in 1934. They were associated with pine, mostly *Pinus pinea*, and were fruiting most plentifully in October. In Africa they reach a maximum in January and February. Other hosts or symbionts are: *Pinus halepensis*, *P. pinaster*, and *P. pithyusa*. In mixed stands of these Mediterranean pines and *Pinus silvestris* some intermediate forms may be observed, thus the soil also may play an important rôle in this. Besides, old specimens of the Mediterranean race often are hard to distinguish from *S. granulatus typicus*.

The American form, in contrast with the European forms, grows with *Pinus strobus*, at least preferably so. The Chocorua specimens, observed by D. H. Linder and the writer, were obviously connected with this five-needle pine, and occurred as well in pure pine stands as in mixed woods with scattered pines. However, some of the fruit bodies were too far from any pines to assume connection between their mycelium and the pine roots. They grew in a *Fagus grandifolia* stand with intermixed *Tsuga canadensis*, and were obviously connected with the latter. They were not otherwise different from the pinophilous majority. In Newcomb Co., N. Y., where the boletes were likewise found to be connected with *Pinus strobus*, a similar ecological anomaly was observed when a form similar to *S. granulatus* in a few rare cases grew far from pines in a very open stand of *Populus tremuloides* and *Salix* spec. with numerous intermixed firs (*Abies balsamea*), and was clearly connected with the latter. But in this case, the fruit bodies seemed to be morphologically modified: they were somewhat larger than the average pinophilous plants and the stipe was pure white as in **Suillus brevipes** (Peck) Sing. comb. nov. (*Boletus brevipes* Peck), and not dotted-glandulose in its lower third to three-quarters, and only minutely granulose in its upper portion; it was rather elongate and slightly attenuate or swollen below; the margin of the pileus was strongly fibrillose-tomentose, probably from a rudimentary veil as occasionally observed in *S. brevipes*; the tubes were strongly decurrent as in a certain form of *S. brevipes*, growing under abnormal ecological conditions (in dense mesophytic hammocks) in Florida (this form was described as *Boletus pseudogranulatus* Murr.); the context showed reddish discoloration. All this points to the conclusion that the American form of *S. granulatus* is not, as the European races probably are, at least when growing spontaneously in nature, confined to very definite soil conditions and to a certain limited number of hosts (two-needle pines only), but able under certain conditions to pass over to other neighboring conifers, either with slight morphological changes (as in New York with *Abies*[6]), or without any such changes (as in Chocorua). In neither case did the soil contain a considerable amount of lime. In the light of recent

[6] At the present stage of my studies, I do not think that it would be wise to insist on the above determination of the specimens concerned. It is also possible that they were not closest to the American form of *S. granulatus*, but rather an extreme form of what Peck called *Boletus albidipes*.

publications it seems probable that the western white pine (*Pinus monticola*) is another symbiont of *S. granulatus* ssp. *Snellii*, a relationship which, considering the taxonomic position of this pine, would not be surprising at all.

As for the ecological characters of *S. placidus* it is well known that this species grows in symbiosis with two five-needle pines, *P. strobus* and *P. cembra*. It thus appears to be closest ecologically to *S. granulatus* ssp. *Snellii*, not ssp. *leptopus*.

Consequently each of the races of *Suillus granulatus* evidently has, in nature, its own requirements as to mycorrhizal hosts. Since these hosts have very definite geographic areas, the fungi concerned have them also. All this corroborates the statement that there are myco-ecological as well as geographic races for which we use the taxonomic term subspecies. *S. placidus*, however, not having a host of its own, yet morphologically and chemically more differentiated than the other forms discussed, must be treated as a separate species. I am aware of the fact that many taxonomists prefer to call forms like ssp. *Snellii* autonomous species rather than subspecies. More detailed information on **Suillus albidipes** (Peck) Sing. comb. nov. (*Boletus albidipes* Peck) will be needed to decide whether in this case we have to deal with a group of small species, or rather with a complex species consisting of several subspecies, possibly including even *S. albidipes* and *S. brevipes*.

II. REDESCRIPTION AND NOTES ON BOLETUS RUBINELLUS PECK

Macroscopical characters: **Pileus** "light Corinthian red," "Corinthian red," "Etruscan red," "deep Corinthian red," and often reaching "Indian red" in the middle, often more brownish tones mixed in when old, or bleaching to a uniform pale yellow, but normally only the margin, if at all, light yellow to yellowish white, warty-floccose, occasionally almost squarrulose-verrucose on the disc, more faintly tomentose on the margin, the flocculae more or less detersible, subviscid when wet, dry in dry weather, the margin sterile (about 0.5–2 mm.), broadly conical or convex, eventually often broadly umbonate, 10–50 mm. broad. — **Tubes** (bluish) pink to salmoneous, or dingy reddish, eventually with a brownish tone, adnate or depressed around the stipe, medium long (about 5 mm.), pores initially daedaleoid, later angular, and rather small to rather wide (up to 1 mm. in diameter), sometimes radially elongate near the stipe. Spore print ferruginous-brown. — **Stipe** subconcolorous, often mixed with yellow or yellowish white, subglabrous to slightly pustulate-subflocculose, dry or almost so, smooth or almost so, equal or slightly enlarged near the base or with widened apex, solid, 10–50 x 2–10 mm. — **Context** whitish yellow to brownish yellow, usually more intensely colored in the stipe, or near the base with some pink portions in some specimens; odor none; taste mild. — **Habitat:** On the ground in woods, always under conifers. July to September. Mostly gregarious. Infrequent to locally moderately frequent.

Microscopical characters: **Spores** 9.5–13.8 x 3–4.2 μ, long ellipsoid, some

more fusoid, smooth, thin-walled, brownish to melleous, rather pale; **basidia** 25–32 x 8–10.5 μ, 4-spored; **cystidia** usually ampullaceous or fusoid with a bottleneck-shaped apex, but also subulate or fusiform without an elongate apex, 38–52 x 7–10 μ, the "bottleneck" if present about 3–3.8 μ thick, hyaline or more rarely brown, not as conspicuously incrusted as in *S. piperatus*,[7] rather numerous and occurring on the pores as well as in the tubes; **trama** of the tubes well differentiated in a completely hyaline, more gelatinized and consequently somewhat loosely arranged and slightly but distinctly divergent lateral stratum, and a slightly colored, denser, regular mediostratum, though in mature specimens this differentiation becoming more and more indistinct, and the hyphae more and more parallel, without clamps, but with occasional nodules or bridge-like connections between parallel hyphae; **warts** of the covering layer consisting of long, cylindric, hyaline, thin (1.5–8 μ in diameter) hyphae with thin walls and without clamp connections, their wall more or less gelatinized and imbedded in a gelatinous mass, their free ends obtuse, and sometimes with strange protuberances.

Chemical characters: **KOH** on the surface of the pileus: blackish, then becoming brown, eventually brown-red; on pores and tubes: dirty brown; on context: tawny brown. — **NH_4OH** on the surface of the pileus: yellow, then becoming olive yellow; on the pores and tubes: deeper pink, eventually gray. — **Aniline oil** on the surface of the pileus: characteristically rapidly deep grayish purple. — **Formol:** no reaction.

The above description has been drawn from collections made by D. H. Linder and R. Singer near Chocorua, N. H., and additional fresh material collected by the writer near Newcomb, N. Y. This material has been compared with the type at Albany which macro- and microscopically proved to be identical. The type of *Boletus Vanderbiltianus* Murr., collected in North Carolina and preserved at the New York Botanical Garden, is the same species. It is said to grow in thin oak woods, but Dr. H. D. House who collected it there remembers that there were conifers scattered in the woods (oral communication). On the other hand, Coker and Beers (*2*), describe and figure (p. 38, *pl. 3, fig. 2–3*, and *pl. 62, fig. 13–14*) this species in a way suggesting that some other species must have been mistaken for *Boletus rubinellus*. This opinion is in accordance with the fact that these authors do not recognize *Boletus Vanderbiltianus* as a synonym of *B. rubinellus*, but say, instead, that they have not collected the former. Furthermore, in a note concerning their conception of *Boletus rubinellus*, they suggest that it "may be only a form" of *Suillus piperatus*. This is probably correct as far as *B. rubinellus* sensu Coker & Beers is concerned, but in our opinion their species is the same as **Suillus piperatus** var. **amarellus** (Quél.) Sing. comb. nov. (*Boletus amarellus* Quél., *Boletus Pierrhuguesii* Boudier, *Boletus rubrotubifer* Kauffm.), a species which I collected in fresh condi-

[7] **Suillus piperatus** (Bull. ex Fr.) Sing. comb. nov. (*Boletus piperatus* Bull. ex Fr., *Ixocomus piperatus* Quél., *Chalciporus piperatus* Bat.)

tion in the Pyrenees, and which is mild and also has the other characters of the North Carolina fungus. The true *B. rubinellus* is a northern species, more frequent in the Adirondacks and the White Mountains, reaching Kentucky in the West, and the mountains of North Carolina and Tennessee in the South. It does not occur in Europe. As for its affinities, the gelatinized surface hyphae and the color of the tubes and spores, as well as its habitat near conifers suggest a close affinity with the section *Piperati* of the genus *Suillus* (*Ixocomus* sec. *Piperati* Sing. 1938, genus *Chalciporus* Bat. 1908). This relationship is confirmed by the anatomy of the tube walls which is quite typical for a *Suillus*. The chemical reactions also agree with those indicated by Singer (9) for this group. Consequently, the new combination **Suillus rubinellus** (Peck) Sing. is proposed.

III. On Lactarius nigroviolascens Atk. ex Burl.

Macroscopical description: **Pileus** dark brownish gray or dark grayish brown to almost black, opaque, pruinate-tomentose to strongly velutinous, dry, smooth, convex, eventually depressed, but usually only in the center, and, at the same time, showing a small but distinct papilla, especially when young, 33–73 mm. broad; the margin frequently short-sulcate or coarsely crenate as in many Leucopaxilli. — **Lamellae** light ochraceous yellow, arcuate sickle-shaped, rather narrow: 3–4 mm. broad, acute in front and behind, not anastomosing, moderately close to subdistant, polydymous, simple, decurrent or adnate but from the point of attachment down the apex of the stipe proliferating into very short and low parallel ribs. Color of the spore print on white paper yellow, "F" (Crawshay). — **Stipe** concolorous, whitish at the extreme base which is buried in moss, versiform but usually subequal or equal with only the extreme base tapering downward, faintly tomentose-velutinous, solid, 50–75 x 7–15 mm. — **Context** white, becoming dark violet when wounded. Latex white, showing dark violet spots when exposed to the air in contact with the context. Odor not significant. Taste mild. — **Habitat:** Coniferous wood or mixed wood under conifers, on moss or sometimes on naked soil.

Microscopical description: **Spores** 8–13–(15) x 7–11.5–(13) μ, subglobose, with conspicuous 1.5–2 μ high ridges forming an ornamentation of the type II or IIIa, more or less asymmetric; in authentic print from N. Y., 10.5–11.7 x 8.5–10.5 μ with a moderately dense to rather loosely arranged ornamentation of type II to IIIa (as above), or IIIb, more rarely IV or V, the spines and ridges usually connected by an extremely faint network which is easily overlooked, 1.3–2.3 μ high; **basidia** 27–46 x 9–13 μ, 4-spored, but very few 2-spored ones intermixed; **cystidia** none; **cheilocystidia** on the heteromorphous edge of the lamellae versiform (cylindric, clavate-balloon-shaped, fusoid, or basidiomorphous), hyaline, 14–52 x 3.3–8 μ; **cuticle** of the type called Virescens-type in *Russula*, consisting of a subcutis of globose cells (about 17–20 x 15–17 μ) which mostly proliferate into a subulate, more rarely clavate or cylindrical hair with narrowed or broadly rounded apex and 0–2 septa (not counting the wall between the

subcuticular cells and the hair); these hairs, 20–120 x 6.5–9 μ, are strictly erect and form the epicutis; pigment grayish brown, dissolved in the cell-sap; all hyphae without clamp connections.

OBSERVATIONS ON THE ANATOMY OF THE CUTICLE, THE SPORE WALL AND THE HISTORY OF THE SPECIES

The above description shows that this interesting and rare species belongs in the section *Plinthogali* Burl., because of the structure of the surface and the combination of the macroscopical characters. The structure of the cuticle, strongly reminiscent of what is known as Virescens-structure in the genus *Russula*, has never been described correctly in any species of *Lactarius* where it is characteristic for the whole section *Plinthogali*. We have observed this structure in *L. fuligineus*, *L. azonites*, *L. lignyotus*, *L. acer*, *L. nigroviolascens*, a southern species with unchanging latex, and a new Florida species with yellow watery latex. Unfortunately, Neuhoff in his work on the "Milchlinge" (7), misinterpreted the structure of the *Plinthogali* by taking the cellular subcutis for the outer layer, and the hairs for the inner layer. Nothing like this, however, can be observed in any European or American *Lactarius*. The spores of *L. nigroviolascens*, as described above, seemed to be more cristulate than the description given by G. S. Burlingham (1) would indicate. However, Miss Burlingham was kind enough to send me some authentic material, probably from the type locality, or not far away from it, consisting of a good spore print obtained from a specimen that she herself collected near Ithaca, N. Y. If the extremely faint connecting veins found here between the warts and ridges are not taken in consideration, at least a minority of spores of this print shows about the same marks as two of the three spores figured (l. c., fig. 3^2). It must, therefore, be admitted that the ornamentation of the spores of *L. nigroviolascens* Atk. ex Burl. varies to a certain amount not only in different collections but also occasionally in a single print. The uniformly cristulate spores were observed in all our New Hampshire material. Farlow had it from Chocorua, Shelburne, and other places in New Hampshire, and his collections remained partly undetermined, or were tentatively determined as *L. lignyotus* Fr., a closely related but different species, easily distinguishable by the pink to latericious color of the wounds. Fresh material in excellent condition was collected by D. H. Linder and R. Singer in the same region where Farlow collected this species for the first time, i.e. in 1891, and 1910. Farlow had the essential data on this species, and gave correct notes with the specimens, indicating how it differs from similar Lactarii. Though realizing it was different from all species known to him, he did not describe it as a new species. At that time, many mycologists would have described a species on far less evidence than Farlow had, but Farlow was much more conscientious in this regard. Incidentally, Atkinson, who collected his specimens many years after Farlow, did not describe them either, but gave them the herbarium name *Lactarius nigroviolascens* which was later (1932) validly published by Miss Burlingham.

IV. A NEW SPECIES OF RHODOPHYLLUS: R. FARLOWII

COLOR PLATE

Rhodophyllus Farlowii Sing. spec. nov.

Pileo laete aurantiaco-luteo, non fortiter hygrophano, convexo, umbilicato, ad marginem pellucide striato, levi, glabro, haud viscido, 24–40 mm. lato; lamellis salmoneis vel pallide salmoneo-latericiis, subangustis vel moderate latis, adnatis vel dente decurrentibus, confertis vel subconfertis; stipite concolori pallidioreve, glabro, levi, sicco, cavo vel farcto-subcavo vel solido, aequali vel ad apicem subattenuato, 50–76 x 2–6 mm.; carne alba vel subconcolori in adultis, inodora; habitatio ad ligna et humum in silvis montanis et in paludosis, aestate.

Pileus rich orange-yellow, more yellow between the deeper orange striae, tending to bleach from the center outward, and somewhat hygrophanous, convex with a distinct broad umbilicus, the margin transparently striate about halfway to the center, smooth, glabrous, not viscid, 21–40 mm. in diameter. — **Lamellae** salmon-pink to salmon-pale-lateritious, 2.5–5 mm. broad, broadest in the middle or in the inner third, adnate with decurrent tooth or subdecurrent to decurrent, close or subclose. Spore print pink. — **Stipe** concolorous with the pileus, or slightly paler, the base more whitish, glabrous, smooth, dry, hollow or solid, or stuffed-subcavous, equal or slightly tapering upward, 50–76 x 2–6 mm., usually about 4 mm. broad at the base, and 2–3 mm. at the apex. — **Context** white, at times somewhat concolorous when old. **Odor** insignificant. — **Habitat:** On wood or on humus in mountain forests and swamps, under **Tsuga** and other trees in mixed stands. July and August.

Microscopical description: Spores 7–10 x 5.2–6.3 μ, pale pink, approximately hexagonal and the shape of a benzine ring when seen frontally, or the sides odulose and the spores thus becoming symmetrical-complex, their hilar portion forming an angle of about 90° when seen frontally; basidia about 50 x 14 μ; cystidia not seen.

This beautiful species is easily mistaken for a *Hygrocybe* (Hygrophoraceae) because of its color and habit. It has been found by D. H. Linder and the writer in July, 1941, near Chocorua, N. H., and was compared with dry material preserved at the Farlow Herbarium, collected by W. G. Farlow himself near Chocorua in August 1906 ("Kr. 229"). Another collection was made by Davis at Stow, Mass. ("Kr. 280"). Both were painted by L. C. C. Krieger and are unmistakably the species described above as the northern form. Then, in 1942, the writer twice collected what seems to be the same species in Highlands Hammock State Park in Florida. These southern specimens are sometimes of a pure yellow rather than orange, and the pileus is perhaps somewhat smaller. Though these differences are slight and not striking, and the microscopical characters are the same, it seems wiser to describe them separately.

Pileus "light orange yellow" to "deep chrome," or more yellow without an orange tinge: "light cadmium" with the margin "mustard yellow,"

eventually much more sordid and bleached: "honey color," margin "cartridge buff," hygrophanous, but not very strongly so, transparently striate on the margin, convex with narrowly depressed disk, later infundibuliform, smooth, glabrous, not viscid, 13–22 mm. in diameter. — **Lamellae** "pale orange yellow" or (in the yellow specimens) "maize yellow," bleaching to "cartridge buff" or more pinkish from the spores when old, horizontal, decurrent, rather narrow, subclose. — **Stipe** subconcolorous with the pileus or "lemon chrome" at the apex and gradually paler downward, the base pallid or whitish, glabrous, smooth, not viscid, subequal, 40–55 x 2–3 mm. Mycelium white. — **Context** whitish or somewhat concolorous. **Odor** insignificant. — **Habitat:** On the forest soil in low hammock, especially where the broad leaved trees are intermixed with *Pinus palustris*. September. Infrequent.

Farlow did not make an attempt to determine this species, and even the determination of the genus was very difficult at the time. Thus, one collection was called "*Eccilia* or *Clitopilus*" while the other was marked "*Leptonia* spec. vel. *Entoloma*," and this in spite of the fact that Farlow was aware of the specific identity of the collections mentioned. This proves once more how ridiculous the argument is that Fries's classification, though not natural, allegedly has the advantage of being very practical for determination. Here we have the example that a man of the skill of Farlow put two collections of the same species tentatively in four different genera. In the modern scheme used by Romagnési (*in litt.*) and the writer (10), *Rhodophyllus Farlowii* is easily determined as belonging in the subgenus *Leptonia* Fr. *sensu* Romagnési & Sing. In color, this new species comes close to *R. salmoneus* (Peck) Sing. which likewise occurs around Chocorua. Microscopically, however, and also as far as the shape of the pileus is concerned, the new species belongs in a different taxonomic group. Two colored plates, executed by Krieger, and illustrating the specimens from Chocorua and Stow, are preserved at the Farlow Herbarium, and so are the specimens from these collections. The type collection is the one collected in 1941, and this is also preserved at the Farlow Herbarium.

This may be a fitting occasion to recommend the genus *Rhodophyllus* Quél. as a nomen conservandum to the next International Congress dealing with the problems of fungus nomenclature. Not only is this name used by those authors who have proved, by adding valuable new information on the genus (Kühner, Lange, Romagnési), that they are competent specialists, but it also would be historically just to use the name given by the author who first discovered that the Friesian subgenera and Saccardo's genera *Entoloma*, *Clitopilus* (excluding the *C. prunulus*-group), *Leptonia*, *Nolanea*, *Eccilia* and *Claudopus* (excluding the *C. variabilis*-group), are all congeneric. However, the most important reason for recommending that *Rhodophyllus* be conserved is that if the rules are to be followed literally in this case, one of the just mentioned Friesian subgenera must be emended and all the Rhodophylli would become either *Entoloma*, or *Nolanea*, or *Eccilia*, etc. In this case, whatever the "legally" chosen genus may be, the

number of new combinations necessary to transfer the species listed by Fries and his followers in one of the remaining genera, plus the species recently described in *Rhodophyllus* only, would be much higher than the number of new combinations still necessary in order to transfer the species not yet transferred to *Rhodophyllus* (such as a number of American species properly belonging in this latter genus in case of its conservation). This statement may or may not be true if only the number of species and so-called species in Saccardo's "Sylloge Fungorum" are compared, but it is overwhelmingly true if the number of important, widely distributed and valid specific names is taken as a base for comparison. If this question is not taken care of soon, it is to be feared that some McGinty *redivivus* will turn out a hundred new combinations a day, making *Entoloma rhodopolium* an *Eccilia*, or *Claudopus byssisedus* an *Entoloma*, or *Rhodophyllus Farlowii* a *Claudopus*, depending on which combination of names appeals to him at the time. The scores of resulting synonyms will have to be carried along for centuries.

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Rhodophyllus Farlowii Singer

L. C. C. Krieger *pinxit*

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2(1): 53–70 FARLOWIA January, 1945

WILLIAM GILSON FARLOW
PROMOTER OF PHYCOLOGICAL RESEARCH IN AMERICA
1844–1919

Wm. Randolph Taylor

It is not often that an American botanist has deeply influenced two diverse fields of his subject, but William Gilson Farlow is recognized as a dominant figure both in the study of algae and of fungi. His active career started just as the subject of Botany in this country began to be divided into specialties, and men began to concentrate their attention somewhat on a particular field, thus following the example of the Europeans. Farlow was not without the knowledge of general field botany appropriate to his day, but he both in teaching and research was a specialist in cryptogamic botany. He exercised a profound influence in this field. The algae were his first enthusiasm; he never lost his liking for them and it is the purpose of this sketch to outline the development of his prestige among phycologists, and to try to see how he influenced the study in this country. It is not proposed to attempt to give a general biographical account; that has been well done by others, particularly Setchell.

Farlow seems to have gone through his college career with some definiteness of purpose. Biographical sketches by his students and associates indicate that his scientific taste exhibited itself in the active pursuit of botany during his undergraduate days, though this was balanced by a marked skill in music. Probably there was little formal instruction available to him in his future field, although what the academic customs of the day denied the infectious enthusiasm of his teacher, Professor Asa Gray, no doubt supplied. The career of a professional botanist doubtless looked like a rather barren prospect to his parents; he with his keen mind for business affairs no doubt had a fair estimate of it. He pursued what we today would consider an essentially classical training and followed the academic term with two years at Harvard Medical School, where he did exceedingly well. Thereby he equipped himself for a remunerative profession; having, thanks to his family's comfortable circumstances, no urgent need for exercising it, he turned to his favorite science of botany and entered Gray's laboratory as his assistant.

For an insight into Farlow's earliest botanical activities we have available little material. In a day when the keeping of diaries was a common custom, he seems to have abstained, and the correspondence of his college days does not seem to have survived. As a result we have nothing before his adult scientific correspondence to tell us when he became especially interested in algae, though of later scientific correspondence there is an abundance.

We do know that when Farlow joined Gray's Cambridge group he did so not to follow in his steps as a vascular botanist, but specifically to enter the practically vacant field of American cryptogamic botanical training. The study of these plants in this country was the professional concern of almost no one. A very few scattered persons were doing specialists' work on lichens, mosses or hepatics which was excellent, but they did not generally hold positions in institutions of such prominence and facilities that their work could be very widely effective. A man in a leading university, concerning himself with the whole field, and organizing its facilities for general training in this field, was urgently needed. In many of the floras of the day a few cryptogams appeared, but in no group had studies in America begun to give for this country the knowledge equivalent to that available to Europeans. Gray saw this clearly, and he had begun to assemble materials as occasion favored. It seems that he had a small but notable collection of algae, the result in the first instance of his friendship with W. H. Harvey who had held the professorship at Dublin, and J. G. Agardh, who was professor at Lund. With this as an incentive it is not strange that Farlow's first active botanical productivity concerned the algae. While still in his apprenticeship he studied a small collection of Cuban marine algae collected by Charles Wright, and published an account of the Chlorophyceae among them in 1871. This, his earliest botanical essay, quaintly illustrated with negative line-cuts on a black ground, shows his painstaking care. He was not yet familiar with the European literature necessary to make this a critical work, but it does show skill and care. It is representative of a type of research which will appear frequently during his career, the report on materials for some exploring expedition.

Meanwhile, his major research work on marine algae was developing. Farlow had joined a group of biologists who spent the summer of 1871 at Woods Hole, Massachusetts, laying the foundation for a biological survey of the area. The United States Commissioner of Fisheries was looking toward the establishment of a research center and hatchery somewhere along the coast, and Commissioner Spencer F. Baird over several seasons examined intensively the advantages of several sites which had been suggested along the New England coast. A small laboratory was set up on the Lightship Service wharf on Little Harbor. Just what share the botanical part of this survey played in the unfolding plans of the Bureau is not clear. In Farlow's own development it confirmed his interest in the New England coast flora. Knowledge of these plants in this country, such as it was, lay with Professor D. C. Eaton of Yale University. He published very little on algae, but did have at New Haven a small collection of New England material, and some additional material from abroad secured by exchange. The friendly connection between these two students of Gray was very useful. Even at first no doubt it greatly fortified Farlow's beginning algal interests; later under pressure of his work on ferns, Eaton gave over the algal field entirely into Farlow's hands. The first fruits of Farlow's interest in New

England algae was a short note (1872) on the algae of the Atlantic Coast, indicating the general character of the flora and some of the striking species.

By now, twenty years after its appearance, the inadequacy of Harvey's *Nereis Boreali-Americana* was evident. Excellently prepared though they were, and for the times accurate, his three volumes had quite too little field work behind them. The months of his too brief visit, with considerable demands on his time apart from collecting of marine algae, were too short to obtain an idea of the algae of the long American coastlines and so he relied on the collections submitted by a few amateurs to complete his catalogue. Local botanists everywhere soon found that Harvey had inadequately presented the algal vegetation of their particular coast, and there began to appear lists aimed at amplifying the record. However, knowledge of marine algae in America was as yet too ill-advanced to permit these lists in general to be taxonomically accurate. They did signalize the general desire for a more complete account of these plants. Farlow, sharing this desire for more complete studies, brought together his observations in a list published in 1873. This is the true start toward his major algal work. Based especially on his 1871 collections at Woods Hole, but including Olney's Rhode Island algae collected in 1846–48, of which those of 1846 and 1847 had been checked by Harvey, and also 1870–71 materials from Greenport and Orient L. I., this paper also contained records which Eaton contributed of plants from New Haven and Watch Hill, and a Miss Pease from Edgartown and Vineyard Haven. This gave Farlow a very respectable list of 103 plants, with eight others considered rare or new to America. It is a matter of interest that Farlow visited and collected intensively at Woods Hole localities recognizable and productive today, over seventy years later. He made this more than a list; though not giving much of descriptive morphological data regarding the various species, he analyzed the flora in relation to other shores, recognized its peculiarities and the importance of Cape Cod as on the line dividing the northern and southern North Atlantic floras.

The publication of this paper was delayed a year and meanwhile Farlow had gone abroad on the first of his notable visits to European institutions. He went with a considerable lot of algae in hand for comparison. His chief base for the taxonomic study of marine algae in Sweden was at Lund, where C. A., succeeded by J. G. Agardh had developed resources for the study of marine algae unapproached elsewhere. Hospitably received there, he firmly established himself in the good graces of Professor Agardh, who through his life gave generous help to Farlow on every occasion. These Agardhian collections are still intact, the specimens numbered in the original sequence, the very large specimens such as *Ecklonia*, *Macrocystis* and *Durvillaea* on great cardboard sheets as Farlow saw and admired them. At Stockholm he was able to accomplish little on his first visit and went on to Uppsala. Here he enjoyed meeting both Fries, the elder at seventy-eight feeble, but assigning Farlow to his son, now the Professor, to be shown the

University. Professor Areschoug, another phycologist, lived nearby, and Farlow noted the excellence of his extensive microscopic preparations of algal structures. He also visited the tomb of Linnaeus at the cathedral, a matter of sentimental duty to all botanists. After about a year with De Bary at Strasbourg, he went to Switzerland where he collected lichens with J. Müller and, in line with his early general botanical training and his notable aesthetic sense, admired hugely the spread of colors which flowering plants developed on the alpine meadows. The developing facilities for botanical research at Geneva impressed him. He contrasted the attitude here with that in Germany where, he writes with scorn, the "Vegetationspunkt mania . . . affects many of the younger botanists to such an extent that they are quite unfitted for practical work." However, he returned to study under De Bary at Strasbourg and there developed such a respect for the morphological approach to a fundamental knowledge of algae that he nearly always insisted that his students develop research from this angle, urging that they reserve the systematics for a time when, by fuller experience, they were more thoroughly equipped. He himself here began a study of *Pteris*, particularly its apogamy, which resulted in the publication of three papers. These papers were not really very important ones in his career, nor did he return afterward to a similar topic for research, but they signalize his conversion to a particular approach in research for younger investigators.

Another most interesting algal contact which he made on this trip was that with J. B. Eduard Bornet in France. Bornet in his collaboration with Gustav Thuret was in a position of great influence in French botanical circles; scientific skill and judgment here combined with financial resources to produce elegantly scientific work of exceptional excellence, and the result was widely acclaimed. Bornet heartily welcomed Farlow and these men became close family friends as well as scientific collaborators. At Bornet's laboratory he was joined by Rostafinski and Janczewski, Polish phycologists who worked chiefly on morphology. Farlow developed the utmost regard for Bornet, and a very extensive correspondence continued throughout their lives, centering chiefly on comparison of specimens. However, the profits from this friendship with Bornet were to develop slowly; the most immediate results from this European trip was to come from his prolonged residence at Strasbourg.

This visit to biological centers in Europe lasted through two years and was followed by other trips during which he firmly established and extended the high regard in which he was held there, resulting, among other advantages, in generous contributions of valuable reference specimens, and exchanges with his herbarium. We cannot follow his tour further, but must consider the later development of his algal interests in America.

He returned from Europe to Harvard, and an appointment which from 1874 was primarily at the Bussey Institution, although he gave instruction in cryptogamic botany at the Botanical Garden in Cambridge, and in 1879 transferred to Cambridge as Professor of Cryptogamic Botany. However,

the brief contact with applied botany at the Bussey Institution induced him to attempt some studies and publication on certain disease-producing fungi, and so began a shift of interest which, though slow to absorb all his research attention, eventually did so.

During this time Farlow worked on the project of a catalogue of American marine algae foreshadowed by his first brief papers. His collections, thanks to his correspondence with Dr. C. L. Anderson and others on the west coast, as well as with all phycologists on the east coast, and his friends abroad, had grown to the extent that he felt accurately informed about these plants. Many specimens had been verified during his conference with Agardh at Lund, and others were sent to him from time to time. In 1873 he published his list of the algae of the south coast of New England, and in 1875 after his return from Europe he expanded this to a list of United States marine algae, adding especially west coast and Florida records, totalling 430 items. This was a list with a short general introduction and a few notes, a less elaborate form of the same appearing the next year under the auspices of the U. S. Fish Commissioner with the addition of notes upon economically useful seaweeds.

A small accumulation, especially of California and Florida algae, was published with a number of new species in 1877, but he never developed this general project further. Instead, he took New England records out of the 1875 list, provided a very useful general introduction, descriptions to the major categories and the species, and in 1881 published his *Marine Algae of New England and the Adjacent Coast*, the work which closed his major algal productivity. This paper well deserved its long career of usefulness; clearly presented and complete for its time, it was the uncontested reference work for its area for over fifty years. His fame as a phycologist chiefly rests on this work.

So much for the general main line of Farlow's algal evolution. What other lines of productivity did he develop? It would seem that we can group them into three classes. First in importance was the exsiccata which he founded and issued with the aid of D. C. Eaton of Yale and C. L. Anderson of California. Second was a group of papers of considerable importance locally, dealing with algal contaminants of Boston's water supplies. Third, a group of minor algal taxonomic papers. Almost all his remaining work falls into one of these three classes.

First we may deal with the *Algae Americanae Boreali Exsiccatae*. This appears in his correspondence but little before it was actually produced. Probably he talked it over with Eaton on some visit, so that little was left for written arrangement. However, Eaton (March 5, 1876) refers to a discussion of label size, perhaps for the exsiccata, and in succeeding letters references to specimens in multiple probably have the same significance, while Eaton (September 8, 1877) refers to ". . . enough for the 30 sets with a few I had already," and a week later refers to ". . . specimens too large for the small fasciculus," after which this work entered considerably

into the interchange. The Eaton correspondence was voluminous, that with Anderson less so. Anderson writes (Sept. 16, 1876) "I like your suggestion in regard to publishing an authentic set of our algae. I shall have considerable time this winter that may be devoted that way. I do not know that I have enough of our species to make a set of fifty but . . ." This seems to open up the business of western participation in the series. The fifth and final issue appeared in July, 1889. The importance of the series lies in the good quality of the specimens, the accuracy of the determinations, and particularly the fact that this first exsiccata of American algae now introduced these plants into many important collections at home and abroad, where before probably only the Harveyan and Agardhian collections had any large representation. It did very much to increase Farlow's prestige abroad. He was largely instrumental in starting the project and in the end it was practically under his full control.

The series of five papers 1876–80 dealing with algae in the reservoirs of the Boston water supply and related topics probably called for little critical research, but they were very important in discharge of Farlow's civic responsibilities. These papers at first dealt with a simple problem of unpleasant taste. Farlow found nothing clear-cut in the algal flora that could be specifically responsible. He indicated a few of the most characteristic freshwater algae present, and was able to assure the public that these algae were in no way dangerous to health. Apparently in at least one 'pond' the trouble was due to a *Spongilla*. Finally in the 1880 paper he wrote a semi-popular treatise on the general features of the algal floras of reservoirs.

Lastly one turns to what may best be described as Farlow's shorter algal works. When one inspects the list it is clear that relatively little bears upon his own collections. Farlow travelled and collected extensively on the west coast, in Florida and in Bermuda. The material went into his collections and presumably was used for exchange, but he published few of the new records which it contained. As papers covering material which he had collected himself, or which at least came from an area and a flora familiar to him, we may recognize his *Notes on New England Algae* in 1882, in 1889 *On Some New and Imperfectly Known Algae of the United States I*, and in 1899, *Three Undescribed Californian Algae*.

The prominent feature of these shorter papers is based upon his commanding position in American botany. While the era of great unspecialized natural history exploration was passing, nevertheless it had not disappeared. Parties were still going off and bringing back heterogeneous, often small, often inexpertly collected lots of material to be parceled out to various specialists for study. As always the algae were a minor part, assembled very clumsily, but they represented a cherished bit of the treasure of the often arduous journey, and they could only be entrusted to Dr. Farlow. So, because he greatly liked these plants and felt it his duty to care for these hard-won little collections, he studied them, occasionally reported new species, and published an annotated list. The geographical range covered is very great, though sometimes there were as few as a half-dozen

species included. His *Cuban Seaweeds* of 1871 was followed by reports on algae from Kerguelen Island in 1876, Cumberland Sound in 1879, Texas and northern Mexico in 1883, Alaska in 1885, Ungava Bay in 1886, South America in 1888, Peary Arctic Expedition in 1895, Azores in 1897 and the Galapagos Islands in 1902.

Farlow's last strictly algal paper appeared in 1916, a very scholarly discussion of the general character and distribution of Pacific Ocean algae, showing that his interest had persisted through the years. His last recorded paper was directly connected with algae too, as it was a biographical sketch of his friend J. B. E. Bornet, published in the same year.

One of the chief circumstances of Farlow's career and one of great potential importance for the development of his phycological interests was his share in the development of the Marine Biological Laboratory at Woods Hole in Massachusetts. The prospectus for this organization was dated 6 April 1887, and Farlow was the Trustee first named in this document. He desired to resign the following year, but letters from Minot and others urged him not to do so, and he served for some years before he severed his connection. During this period of development under the directorship of Professor C. O. Whitman of the University of Chicago, the Trustees were called upon for much financial help. Eventually some came to feel that they were losing control of the finances of the institution, that its expenditures were exceeding what the Trustees should be called upon to bear, and that the limitations which they set upon the activities of the Director were disregarded. Director Whitman clearly had a larger vision than these Trustees, a vision less trammeled by concern over details of economical operation and balanced budget. A group of the Trustees resident near Boston were particularly resentful of the attitude of Whitman and the business management of the Laboratory. Having organized and financed the institution, they felt that they should direct its administration. On the other hand, in the course of ten years the biologists who formed the Corporation, who actually came to work at the Laboratory, now felt that the institution was peculiarly their own, that they could shape, expand, and staff it as they chose, and that the Trustees who were commonly not persons working at the Laboratory during the season, should defer to the general desire of the active group.

The meetings of the Corporation and Trustees had been held in Boston in the winter and most of the Corporation members could not attend; a strong desire for greater participation of the biologists from all over the country led to a demand that meetings be held at Woods Hole during the summer. This was done in 1894, when a meeting of the Trustees was held there, but the Annual Meeting at which the major business was transacted was still held in Boston. The actively dissatisfied group of Trustees, some seven or eight out of twenty-one, engaged in an exchange of bitter letters among themselves, looking toward more effective control of the Laboratory. A special Corporation meeting was called at Boston, 16 Aug. 1897, to pass

by-laws changing the Annual Meeting to Woods Hole, and the date from January to August. This meeting was presided over by Farlow as Acting President; he tried to show that the meeting was illegally called, but failing of support, withdrew. The meeting proceeded under C. G. Kidder to make important changes in the by-laws, improving greatly the scheme of organization. However, seven of the Boston group of Trustees afterward resigned. Six of them, and one other dissatisfied Trustee, attacked the administration of the Laboratory in a very severe article in the journal *Science*, and to this Farlow was a party. The correspondence in Farlow's file shows the stages in the development of this *Science* article, the anger of the participants, and Farlow's share in keeping the text within reasonable bounds. The supporters of the Whitman administration replied in a privately printed pamphlet, subsequent to which, with the withdrawal of the chief dissidents, the whole matter quieted down.

Thus ended Farlow's direct part in what has evolved into the most ambitious project of coöperative biological organization ever attempted. It remains for us to enquire into his share in the work of the Laboratory during his trusteeship, and the persistence of his influence after he resigned. It does not appear that Farlow was ever active in instruction at the Laboratory, or that he was in any large way an investigator there. The first year there was no staff botanist; in 1889 James E. Humphrey was appointed, to be followed the next year by W. A. Setchell. Humphrey had been introduced to the algae by the Rev. J. D. King at the Martha's Vineyard Summer Institute; he studied the development of the perforations in *Agarum* for his B.S. degree thesis at Harvard, though he turned to the Saprolegniaceae for his doctorate in 1892, and eventually he deserted mycology for cytology. Setchell, another student under Farlow, was obviously an appointment favored by him. Setchell returned in 1892 and subsequently through 1895, when he departed for the west coast. Obviously Farlow, already retiring from teaching at Harvard, was content to transmit his algal enthusiasms through his students. In 1896 a greatly enlarged scheme of botanical instruction was initiated under the leadership of Professor J. M. Macfarlane of the University of Pennsylvania, who had been trained at Edinburgh, but here again Humphrey and one of Farlow's students, George T. Moore, guided the work on cryptogamic botany. This brings one to the year of the great upheaval, but in 1897 B. M. Davis, another student from Farlow's laboratory, who had gone to the University of Chicago, took control and with G. T. Moore again gave the cryptogamic work. The greatly expanded list of botanical instruction soon was restricted, but Davis remained in charge, and Moore was associated with him until 1905. During this time various other additional instructors appeared, for algae in particular J. J. Wolfe from 1902–1906, again trained by Farlow. Moore took charge of the cryptogamic botany in 1906 and carried it through 1918; associated with him until 1914 was G. R. Lyman — a Farlow student — and others, including some Harvard-trained men, but probably none who were directly trained by Farlow.

Here, then, ends the immediate influence of Farlow and his students in this venture, which has grown from a handful of investigators in 1888 with most trifling equipment, to a group averaging over 360 for the five years before the present war, and about 125 students, with extensive equipment, large permanent buildings and a considerable maintenance staff. By 1900 it had become clear that the Laboratory was not to collapse; in spite of the adverse judgment of Farlow and his friends for which there was some justification, the administration rallied scientific and financial support, and went its way. Farlow never took further active share in it; neither, however, did he dissuade his students from doing so and under their guidance its botanical work prospered for thirty years. While it is extremely doubtful that Farlow actually was diverted from algal study to any great degree by the Woods Hole dispute, it is clear that for other reasons he was willing to let others do the research on these plants. By now he had two promising protégés, his student Setchell and an amateur, F. S. Collins, who was an accountant and business man of Boston and Malden. For the rest of our analysis of Farlow's influence we may go to his list of academic students and to his friends, their records and their correspondence with him.

A formal list of Farlow's students does not seem to have been compiled. From the congratulatory volume of photographs and letters sent to him on the occasion of his seventieth anniversary we may, however, glean the names of most of those of his students who specialized in algae at least for a time.

The first man, apparently, to take a doctorate degree under Farlow was B. D. Halsted, who in 1878 submitted his successful thesis on American charophytes. This was the nearest to a taxonomic dissertation that Farlow ever permitted a student to submit. Halsted became Professor of Botany at Rutgers College where his interests changed and he mainly concerned himself with plant diseases and the breeding of crop plants.

However, perhaps an earlier student contact was that with F. W. Hooper, who took a bachelor's degree at Harvard in 1875, but his master's degree only in 1898, and did not proceed to the doctorate. There is no record of significant phycological publication on his part, but he contributed Florida algae to the *Algae Exsiccatae Americae Borealis*. His interests shifted to other fields and he became Director of the Brooklyn Institute.

The next student was a very notable one, and for Farlow his appearance was a fortunate circumstance, because into his hands Farlow consigned with confidence the teaching of cryptogamic botany when his affairs induced him to drop it in 1897. This was Roland Thaxter, who took his bachelor's degree in 1882 and his advanced degrees under Farlow six years later. Thaxter's brief excursion into the algal field included especially his paper on *Compsopogon*, a morphological study. He then turned his attention to the fungi and became a very notable figure in that field.

At this period there came under Farlow's influence Kingo Miyabe, an able Japanese botanist who had had his early training at Sapporo. He took

his doctorate at Harvard in 1889 with his phanerogamic thesis on *The Flora of the Kurile Islands*. In later years he published extensively on the marine algae, and especially the Laminariaceae, of Japan.

While Thaxter was a student, three others were pursuing algal problems with Farlow. J. E. Humphrey took a bachelor's degree in 1885 and went out to teach at Indiana University; he returned to take his doctorate in 1892 with his thesis upon the Saprolegniaceae. He introduced the study of cryptogamic botany and especially the algae at the Marine Biological Laboratory, but his interests shifted to cytology in later years. While holding a post at Johns Hopkins University and in charge of a biological party in Jamaica, he contracted yellow fever and died in 1897.

R. P. Bigelow, likewise taking a bachelor's degree at Harvard in 1887, studied and published upon the structure of the adult growing point of *Champia*. He shifted his interest to zoölogy and held a professorship at the Massachusetts Institute of Technology.

In 1887 Farlow's friend, Professor Eaton of Yale, sent a very promising new graduate to study with him. This was W. A. Setchell who, like Miyabe, continued actively in algal research to the end of his long career. These were the only students of Farlow who did so. Setchell took a master's degree in 1888 and a doctorate in 1890, going eventually to a professorship at the University of California. His publications on the marine algae of the west coast are extensive and fundamental, but he had many and varied auxiliary interests which also led to publication.

G. J. Peirce took a bachelor's degree at Harvard in 1890 and thus came under Farlow's influence, though he went to Leipsic for his doctorate. Peirce was essentially a physiologist, and eventually Professor of Botany at Stanford University. He published on the relation of *Nostoc* to *Anthoceros*, gamete extrusion in *Fucus*, irritability in algae and on brine organisms.

The next year H. M. Richards likewise graduated from Harvard; however he continued and completed the doctorate in 1895. His early algal work dealt among other matters with parasitic algae; later his interest shifted to the fungi, wound reactions and the like. He held a professorship at Barnard College.

From this time on, all of Farlow's students held a bachelor's degree from another institution, although some added the Harvard one to it. B. M. Davis came to Harvard from Stanford University. Taking the master's and doctor's degrees in 1894 and 1895, Davis went on to a detailed study of algal distribution in the Woods Hole area for the U. S. Fish Commission, which was published in 1913. Although Davis's interest in the morphological side of algal studies continued, his actual research interest changed to plant genetics. He held a professorship at the University of Michigan.

G. T. Moore came to Harvard in 1895 and repeated the bachelor's degree, taking the master's the next year and completing the doctorate in 1900. His early interests were particularly in the freshwater algae; later the burden of administration kept him from activity in the field. He be-

came Professor of Botany at Washington University and Director of the Missouri Botanical Garden.

In 1897 E. W. Olive took a master's degree, and in 1902 a doctorate at Harvard. His early interest was in Myxophyceae, particularly their difficult cytology. Later his interests shifted to mycology and he served as curator at the Brooklyn Botanic Garden before leaving academic fields.

J. J. Wolfe came to Harvard, to take his doctorate in 1904, and made a cytological study of *Nemalion* which was well considered at the time. He did not continue with algal research. He held a professorship at Trinity College, Durham, N. C.

The next student at Harvard with claims to specialization in algae was R. F. Griggs. He took his doctorate in 1911 and studied the various features of the west-coast brown algae before becoming interested more in general matters of plant distribution. He holds a professorship at George Washington University.

Finally we may mention F. K. Butters, who took a second bachelor's degree at Harvard in 1900, returning to complete his doctorate in 1917. He studied especially the genus *Liagora* and was interested in Hawaiian algae, but later concentrated his attention on ferns and phytogeography, holding a professorship at the University of Minnesota.

Reviewing this list we see that only two men continued consistently in algal research. Four, namely B. M. Davis, G. T. Moore, H. M. Richards and R. F. Griggs continued profitably with algal work for a time, and then went on to other fields. In teaching no doubt these all transmitted something of their early enthusiasm; to the teaching at Woods Hole, largely algal, Humphrey, Setchell, Davis, Wolfe, Moore and their academic descendents, all contributed.

It is hard to judge the worth to science of these men. Some were active researchers, like Thaxter and Setchell, others administrators like Moore. Of the fifteen names, eleven carried the star with their listing in *American Men of Science*, indicating a considerable recognition by their colleagues; Humphrey died before the custom was initiated, and Miyabe was a foreigner and so not included. In the voting for the first edition, Farlow himself ranked first among botanists, Whitman second among the zoölogists, exceeded only by W. K. Brooks. Seven of Farlow's students were starred in this first edition, one being in zoölogy. Almost all of his phycological students came to hold full professorships in large and well-equipped universities, many being the departmental heads. That the actual output of algal research is not great for such a number over so long a period is probably related to the specialized character of the field and the few openings favorable to specialists in marine algae. The thread of inspiration was not broken, however, but carried on into the next generation through Setchell.

The topic of Farlow's relations with other botanists is too huge to develop fully in the space remaining for this article. One may well doubt if any botanist of his generation or since has conducted as extensive a per-

sonal correspondence. Farlow knew every phycologist of note, and kept the acquaintances active. The lesser men at home and abroad constantly referred material to him for advice; he in turn sent material overseas to persons in a position to give expert help, and thereby kept up the acquaintances established during his trips abroad. Farlow never cared for either an amanuensis or a typist; he wrote his letters, one may judge from some remarks, concise ones, with his own hand and seldom kept copies. The letters reaching him he kept, thousands of them, and in his later years he had them sorted and bound into scores of volumes. A person who had grown up with knowledge of Farlow since college days would be in a much better position than the present writer to extract a connected story of Farlow's life and friends from these letters. Only half of the story is there, the other half must be surmised, for in only such rare cases as those of Gray, Bornet and Collins has the other side of the exchange been brought into the Farlow library. The earliest correspondence certainly is missing, and there is no way of telling how much has been eliminated before what Farlow thought was important was bound together.

In the first place we must recognize that the bulk of the scientific matter in these letters consists of lists of identifications and notes on various plants, discussions of nomenclature and the like. From this, little suitable to the present presentation can be extracted. Of the general material only a few glimpses can be offered, to give an idea of the nature of the problems which were referred to Farlow for advice.

The first group of correspondents are those dating from Farlow's Bussey Institute period and his first European trip. Eaton, Anderson and Wolle represent the main American contacts of these days. Professor D. C. Eaton of Yale University, also interested in the marine algae, was for many years his best and most active correspondent. The exchange opens with a letter from Eaton describing the results of dredging under Baird's direction near Eastport, Maine. He writes that the dredge had brought up *Delesseria sinuosa* and *Ptilota serrata* from at least 400 feet depth. Two years later Eaton seems to have visited Farlow at Woods Hole for a collecting trip. The trip must have been strenuous, or at least ended in a rush, for Eaton accounts for the whereabouts of his own and borrowed attire after what one suspects to have been a classical attack of professorial absent-mindedness: "Dear Dr. ——— I remembered the coat before I left Woods Hole, but too late to reclaim it. Your night shirt I have here, & will presently send it to your Boston address by the Express Co. . . . I suppose you are having a real good time of it now your students have gone." (20 August, 1875). The next year begins a very long correspondence about the exsiccata for which Eaton and Anderson shared the responsibility with Farlow, which includes their own collecting and that of Edward Palmer and F. W. Hooper in Florida. In 1879 Eaton was proposing Farlow for a position at Johns Hopkins University, as he mentions having written to this effect to Mr. Gilman. Nothing came of it. In earlier letters it is Eaton's preoccupation with ferns which calls for apology, but from 1881 it becomes evident that

Farlow's interest was shifting to the fungi. Setchell's name is mentioned first in 1885 at a time when it is evident that these correspondents were building up their bryophyte collections; two years later a discussion of Setchell's plans appears, and in June Eaton refers with pleasure to his appointment at Harvard. Apparently Eaton did not think much of organized scientific groups: in 1893 he writes to Farlow expressing disapproval of a botanical congress: ". . . I have the heartiest possible contempt for the whole crowd of priority-worshippers . . . and shall not feel the slightest obligation to conform to their 'Laws'," and regarding an embryo organization in this country ". . . the longer I think of the botanical society the worse I think." There seems to have been close social contact between the families, and Eaton's daughter kept Farlow closely informed of her father's illness and death in 1895.

Dr. C. L. Anderson was the other partner in the *Exsiccata* enterprise. He writes to Farlow in 1876 expressing enthusiasm for the prospective series, and continues with letters regarding the specimens and labels for it.

Francis Wolle, clergyman head of a Moravian girls' seminary at Bethlehem, Penna., was an indefatigable student of freshwater algae, on which he published largely, his books on freshwater algae, desmids, and diatoms having a considerable vogue fostered by the numerous, if not very precise, illustrations and descriptions. The correspondence with Farlow lasted for about ten years from 1875, mainly with regard to specimens the identity of which was in question. Apparently Farlow distrusted Wolle's determinations and for this reason sent some of his specimens to Bornet for redetermination. From a last letter of 1884 we gather that the edition of the *Desmids of the United States* was limited to 460 copies, a venture with the initial success of which Wolle was quite pleased.

The foreign correspondence beginning in this period centers on Farlow's first European trip, and continues until when toward the turn of the century, these older botanists had passed from the scene. The earliest Agardhian exchange opens when J. G. Agardh (8 January, 1872) welcomes Farlow as a new correspondent, and indicates his desire for west-coast algae, especially Laminariaceae. Later that year, correspondence arranging for Farlow to visit Lund appears and the facilities there are explained. Twenty years later Agardh again writes to Farlow at Paris inviting him to revisit Lund, and in August expresses his pleasure in the occasion after Farlow had left. There was much reference to the notable "*Algae Muellerianae*" from Australia, of which he was sending a series to Farlow. Farlow's marriage in 1900 was greeted with the friendliest expressions and a congratulatory poem.

In 1873–74 we have letters from Farlow to Thuret and Bornet arranging for his visit to Antibes. Farlow was having the traveller's perennial difficulty of fitting his plans into the vacation plans of the Europeans. The correspondence with Thuret was necessarily brief, but that with Bornet was very extensive for many years. In 1874 he wrote to Farlow encouraging him to prepare an introduction to the algae of his region, *i.e.*, New

England, and telling him the gossip regarding the Antibes group of botanists which Farlow had lately left. Of course the correspondence dealt largely with determinations of Myxophyceae. They exchanged their major as well as minor works: Bornet in 1878 writes of sending the *Etudes Phycologiques* and in 1882 of receiving four copies of Farlow's *New England Algae* and of Hervey's *Sea Mosses*. In 1883 writing in reply to an account received of Farlow's trip to Minneapolis, he indicates the part which Longfellow's *Hiawatha* and Cooper's tales bore in forming his ideas of that part of America!

Correspondence with Areschoug, Hauck and very many others of lesser note exists, but lends little to the interest of this account. That with Sir Joseph Hooker is more significant; Farlow could act as intermediary and tell him of the waning health of his friend Professor Gray. Fairly early, in 1878, replying to a request for portraits of botanists, Hooker tells what he can supply and which exsiccatae he has in duplicate for exchange. Finally, in 1907 he acknowledges formal congratulations, received on attaining his ninetieth birthday.

With the next age group the importance of the letters shifts to the American field. Farlow is by now a recognized leader, and while his contact with older Europeans flourishes, he is not particularly impelled to active correspondence with the younger group. However, we find some new names, as contact was developing, for instance, with Hariot and Lagerheim.

Among Americans, we have now the period of development of Collins as a phycologist, Farlow's encouragement of the Curtiss family in botanical collecting, and the establishment of his earlier students. Frank S. Collins was a business man employed by the Boston Rubber Company in charge of their accounting. He had little leisure, but he employed it in ardent botanizing. He was quite as much interested in freshwater algae as in marine plants; from contributing to Dame and Collins' *Middlesex Flora* and the algae of Rand and Redfield's *Flora of Mount Desert*, through many smaller papers, he developed his *Green Algae of North America*. On his *Green Algae . . .* , the *Algae of Bermuda* which he prepared with Hervey's coöperation, and on his spectacularly successful *Phycotheca Boreali-Americana*, largest exsiccata of algae ever issued, his fame chiefly rests. His work was for its day very accurate and scholarly, particularly the smaller papers in which he described the novelties he later incorporated in his larger works. For much of this Farlow provided the critical background, the reference collections and encouragement, although ultimately Collins assembled an excellent herbarium and library of his own. As it was possible to confer personally, the amount of correspondence was all the more remarkable. The letters begin in April 1881, but refer to earlier matters, discussing trips to Nahant and Woods Hole, and the plants collected. In Oct. 1894 appears a sentence, ". . . I want to distribute this species, either in the *Phykotheka Universalis* or in the set which Setchell, Holden and I have been talking of issuing, if we ever get to it; and I don't want to give a new name only to have to take it back again." There is

much interchange at about this time regarding the C. E. Pease and E. Butler collections of Jamaican algae on which Collins was to publish. The queries about labels for the *Phycotheca*, to which Farlow contributed specimens, begin to appear and Farlow writes to Collins in February 1895 acknowledging the receipt of Fascicle I, and in April Collins writes ". . . I have sent out about all the first lot I made up of Fascicles I and II, and am laying out the specimens for another batch of the same." From letters to Setchell in 1908 it seems that Farlow was trying to secure money to bring Collins to the herbarium at Harvard, but that enough was not forthcoming. As early as Oct. 1901 Collins mentions that five or six years earlier he had done a good deal of work toward a book on New England marine algae, and that he now contemplated making it cover the whole east coast; this manuscript was never modernized and completed, but went on Collins' death with his collections to the New York Botanical Garden. This interchange, too long to follow through, only ceased with the death of Farlow in 1919, Collins dying the next year.

Another useful interchange, likewise outside the academic sphere, took place about this time between Farlow and the Curtiss family, Mrs. Floretta A. Curtiss and her son, A. H. Curtiss. Mrs. Curtiss, living in Florida, issued three series of *Marine Algae of Florida* starting in 1895, with many of the determinations by J. G. Agardh. Much earlier correspondence had been exchanged between Mrs. Curtiss and Farlow regarding various Florida algae, starting in 1877. Apparently Mr. Curtiss travelled much for government agencies; he also seems to have collected natural history specimens of all sorts for sale. Mrs. Curtiss especially cared for the algae, which her son rough-dried for her in the field, and which she soaked out and mounted. In 1899 he writes of his mother's partial paralysis and her desire to have her personal collection of algae, which contained much valuable foreign material received by exchange, gotten together for the U. S. National Museum. Three months later her death is referred to, and the plans for assembling the *Algae Curtissianae* into volumes for preservation. As late as 1904 letters were exchanged, but seemingly no more algae were to be expected.

Letters between Farlow and his student J. E. Humphrey began earlier, but extend into this period. What we have from 1888 tells of Humphrey's arrival at Indiana University, the lack of a botanical library, his plans to build up the department and his hope for separate departmental status. In 1897 the letters came from Johns Hopkins University, his last post. A curious exchange here illustrates the lack of centralized authority at the Marine Biological Laboratory at the time. Humphrey had been teaching the algae course there; he was preparing for his last Jamaica trip and was asked to secure a substitute. Humphrey arranged with H. M. Richards to come, but Director Whitman without consulting Humphrey or Farlow, engaged B. M. Davis and printed the announcement. Farlow and Humphrey were indignant at having the matter taken out of their hands without warning; Humphrey died on this Jamaica trip, but both Davis and Moore served on the Woods Hole staff repeatedly. This was no doubt one

of the items which collectively induced Farlow to sever his connection with the laboratory.

Except for the Collins correspondence, that with Setchell is the most important domestic correspondence relating to American algae. The first letter is one dated December 1885. Through Eaton, Setchell had sent a specimen to Farlow for identification which proved of interest, and Setchell was forwarding more of it. Then follow a series of letters dealing with *Lemanea, Batrachospermum* and *Tuomeya.* There intervenes next apparently a period when Setchell was ill with rheumatism and spent some months at Sharon Springs, N. Y., and did some field botanizing about there, but with land plants rather than algae. The work at Woods Hole comes up for conference in 1891, and various persons as Davis, Brannon and Rothrock are mentioned as students who later became well known in the botanical world. A perennial topic of discussion still, the Marine Biological Laboratory "mess" is at that time reported as somewhat better than it had previously been, but Setchell complains of the high room rent: $1.00 a week each, two in a room in the Gardiner cottage! Each summer there is more about Woods Hole, and in July 1894 he mentions settling into the then new Botany building. Next June from Yale he writes to ask Farlow to recommend him for appointment to a professorship at the University of California at Berkeley; this after testing the disposition of Yale to meet the competition and getting no encouragement, he reiterates, and tells that he will receive $3000 as department head. In December he writes from Berkeley of the pleasure at the welcome he received, says he has about 500 volumes in the botanical library and tells what other funds are available to him. M. A. Howe and W. L. Jepson were there as instructors; but Setchell felt that since Howe's interests were similar to his own, that Howe should go elsewhere so that the activities of the department might be diversified; he recommended him highly to Farlow and asked help in placing him. The early issues of the *Phycotheca* came in for discussion and Setchell writes appreciatively of Farlow's review in the *American Journal.* Later in 1896, when the large expansion of the botanical lecture list at Woods Hole occurred, Setchell wrote with some amusement ". . . I understand that Macfarlane wants a botanical garden at Woods Hole &c — &c. . . ." Apparently Setchell doubted the wisdom of such ambitious plans. Setchell wrote to Farlow in some detail of his trips abroad, as to New Zealand and Europe in 1905 and again in 1911, and of the algae he collected and those he saw at the British Museum, Kew and the Linnean Society's rooms.

M. A. Howe opened correspondence from Berkeley with a question on *Fucus evanescens,* and asks help on the identification of Pacific Grove algae. He mentions his paper on California hepatics as his doctorate thesis and as a collateral interest. In 1894 he wrote inquiring of any opening at Harvard and in 1898 there were some moves toward bringing Howe to the Harvard herbarium in charge of bryophyte work. The financial terms were set forth at some length, Howe being very explicit regarding what he

felt were the conditions under which he could come. Nothing resulted, however, and other applications to Missouri and Oberlin which he mentions likewise having petered out, he wrote hopefully to Farlow regarding obtaining the headship of the department at Berkeley, which went, however, to Setchell. The latter encouraging him to move, in 1897 he writes of having taken an assistantship at Columbia University. After a break, the correspondence begins actively again from the New York Botanical Garden, and a great many inquiries regarding various plants occur, as finally those during 1919 regarding the Bahama Flora. Howe was eventually the Director of the Garden, and his work on marine algae, while less extensive than Setchell's, was at least equal in quality.

It is not possible to follow the correspondence with European botanists through this period, but one must not fail to notice the friendly interchange with Alexander Gepp as it covered the period of his marriage to Ethel S. Barton, another English phycologist, her later very precarious condition due to tuberculosis, and death. An amusing reference to the compilation of the British Museum of Natural History catalogue occurs, which Gepp describes as having been made up by J. Britten, himself, Wiltshear the attendant, with two words contributed by George Murray. In Gepp's own words (5 July, 1905) "We were the scribes, & George Murray was the Pharisee who contributed but 2 words — viz his own name."

It is impossible for a person who had no personal acquaintanceship with Farlow to write as understandingly of his witty, friendly personality as his own students. One could hardly improve on the charming biographical account by Setchell [1] to which all should refer for details of his life and the complete list of his publications.

Without question Farlow dominated algal study in America for a considerable period, and represented it before European scientists. Also unquestionably this high regard was justified; the problem is to appreciate truly how this came about. It was not due to the quantity of publication which, respecting algae, was never great and for the later years very small. Nor was it due to the importance of the material studied, for with the exception of the New England report it was never comprehensive, nor in general were many new species described. It did not derive from the sumptuousness of publication, for his best work appeared in government reports, nor the elegance of the illustrations, which when from his pen were few, stiff and labored rather than skillful. So far as his publications go they, by the care in preparation, clarity and lack of error, confirm rather than accent his reputation.

One must recognize that the high esteem in which Farlow was held was due to other factors. In the first place he was skilled in gathering together collections. He developed a personal herbarium and library of rare excellence, including items scarce and costly even at that day. His collections

[1] Setchell, W. A. 1927. William Gilson Farlow 1844–1919. Memoirs Nat. Acad. Sci. **21** (4): 1–22. Portrait and complete bibliography.

were in only secondary degree the result of his own field work. Again excepting New England, where at a few places he did get a variety of material, his collecting was not extensive and did not contribute largely to his publications. Rather by purchase and exchange did he secure from others splendid representation of the flora of many parts of the world. These treasures he was quite willing for others to consult, under strict supervision. As a result the more active descriptive phycologists, such as Collins and Setchell, came constantly to him for consultation, appreciated his careful assistance, and spread his fame. In the same way his European travels and active correspondence served abroad.

Here then we have probably the clue to Farlow's most significant contribution. He developed the first good reference collections for algal study in this country. He published skilfully, if not abundantly. He encouraged several young investigators to attempt algal problems, gave them rigorous training in meticulous research, and the two or three most productive among these he assisted in every way, so that they in turn dominated algal studies in America after Farlow's own interests were largely forced to the fungi. Professor Farlow was, then, clearly more important as a promoter, developing resources and trained men, than he was as an investigator himself, and as such he fills a distinguished place in American botanical history.

For the hospitality of the Farlow Herbarium and Library, for access to Professor Farlow's extensive correspondence, and for patient help in using these treasures, the writer is greatly indebted to the kindness of the curator, Dr. D. H. Linder.

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FARLOW'S INTEREST IN AN INTERNATIONAL ABSTRACTING JOURNAL

Frans Verdoorn [1]

In collecting data about the history of international relations and coöperation in the plant sciences, I have gathered a large number of letters written by the late W. G. Farlow. Dr. Farlow, though a charter member of the *Association Internationale des Botanistes*, took no great part in its activities. He has never been a member of the council or of one of its commissions. Yet, during his fruitful life he was in touch with more men abroad than most of his contemporaries and was particularly active in undertaking and developing exchange relations. He was greatly interested in various aspects of abstracting and listing the new botanical literature of the world. In this field he exercised probably more influence than is realized. The need of establishing a truly international botanical abstracting journal was felt and discussed by him at an early date.[2] Dr. David Linder has suggested that it would be useful to publish in this special number of *Farlowia* a few of Farlow's letters on this subject. They are not only of interest for the student of international relations but throw an interesting light on Farlow's attitude towards a number of other problems and persons.

Since 1890 dissatisfaction had been growing, both in the Old and the New World, about the way the *Botanisches Centralblatt*, at that time the only abstracting journal in our field, was being conducted.

In October 1900, Goebel, who had already repeatedly viewed his opinion about the matter and who had even been planning to establish a second abstracting journal in Germany, met Lotsy in Ambach and discussed the situation with him. Lotsy was much interested and subsequently held conferences with R. von Wettstein in Vienna, with M. Raciborski in Lemberg, and with S. Nawaschin in Kiev. In November 1900 he wrote Goebel about a definite plan to start a new international botanical abstracting journal, as well as an international association of botanists which would not only edit and publish the journal, but would also undertake many other things.

In the United States there was at that time no less dissatisfaction. In an address (December 27, 1899) to the *American Society for Plant Morphology and Physiology*, Dr. Farlow had spoken in detail about the need of either changing the *Botanisches Centralblatt* or establishing a new journal. A committee was then established consisting of Dr. Farlow, Dr. D. T. MacDougal and Dr. von Schrenk. This issued a first report on June 27, 1900

[1] The author is under considerable obligation to F. O. Bower, Sc.D., LL.D., F.R.S., of Glasgow and to William Trelease, Sc.D., LL.D., of Urbana, Ill., for their help in securing data about the history of international relations work in botany and for their permission to reproduce the letters reprinted in this article.

[2] Dr. J. C. Bay's earlier efforts in this field are not discussed in this article.

and a definite report on December 27, 1900. The committee, and Dr. W. F. Ganong, at that time secretary of the society, had been very successful. They obtained from Dr. O. Uhlworm, the editor of *Botanisches Centralblatt*, various promises, described in detail in Dr. Farlow's letter of February 4, 1901, reproduced below. As we will see from the following letters, Dr. Lotsy and his group went on with their plans and established the *Association* at a meeting in Geneva in August, 1901. The society acquired the *Botanisches Centralblatt*. We reproduce now three letters by Dr. Farlow which throw some interesting light on the events of that time. The first letter is addressed to Dr. Bower of Glasgow, an old friend of Farlow and also a friend and supporter of Lotsy and his work. The second letter is addressed to Dr. Trelease, a former student of Farlow, who was also much interested in botanical bibliography and who had undertaken to share in the work of the American editorial committee of the *Botanisches Centralblatt*, as referred to in Farlow's letter of February 4, 1901 to Dr. Bower. The third letter is again addressed to Dr. Bower. Lotsy's letter to Farlow, of which a copy is in the Chronica Botanica Archives, is not so impulsive or naïve as one may expect after Farlow's reaction. Lotsy was planning something much bigger than only a journal, he was dreaming of an international botanical bureau, as many others after him have done. Farlow was only interested in the journal and seems to have had little sympathy with Lotsy's plans and way of thinking.

Cambridge
Feb. 4, 1901.

Dear Prof. Bower,

I suppose that last June you received a copy of the report of a committee of the Society of Plant Morphology and Physiology appointed to consider the best means of securing a good record of current botanical literature. A second and final report was sent you about a month ago.

Assuming that the reports were received by you it is not necessary for me to repeat their contents now. The last report was presented by me as chairman of the committee at the meeting of the Soc. Plant Morph. in Baltimore on Dec. 27.[3]

After I returned from Baltimore I received a letter from Lotsy which took me entirely by surprise. I did not know that there was any project for a new journal of reviews in project. About six months ago, I believe, I had a letter from Goebel in which he expressed great dissatisfaction with the Centralblatt and said that he had sometime thought that he would like to start a new journal. But from what he said I did not understand that there was any intention of doing so now and the project seemed to me to be all in the air. Up to June of last year the reply of the editors of the Centralblatt to our suggestions for improving their journal had not been satisfactory to us but at the meeting of last June our committee was directed to continue negotiations with the Centralblatt and, if they would not do what we wanted, to present a plan of a new journal at the meeting of Dec. 27, 1900.

Early in November the editors replied agreeing to do all that we had asked

[3] *Cf.* Farlow in Bot. Gaz. **29:** 64. 1900. A further report was issued in 1901 by Farlow, MacDougal and von Schrenk.

them to do. They decided to put all their worthless articles into the Beihefte and to put all the reviews and the list of new works into the Centralblatt and also to allow one to buy the Centralblatt without the Beihefte (which nobody wants) at the old price. They also asked us to select American editors whose names should appear on the title page to see that American notices were kept up to the mark. They furthermore said that they hoped to have similar editors appointed by botanists in other countries.

Such an arrangement seemed to us at Baltimore to be a good one and a committee was appointed (of which I am a member) to inform the Centralblatt that their proposition seemed satisfactory and to nominate American editors. In fact, we have already selected one editor and have been considering other candidates.

You will imagine my surprise *after* returning from the Baltimore meeting on hearing from Lotsy [4] that a new journal was proposed and he wished me to join his International Association. I should be glad to join the Association were it not that its main object appears to be the starting [of] a new journal in opposition to the Centralblatt. Having, in perfectly good faith, gone so far in our negotiation with the Centralblatt it seems to me that it would not be honest for me, no matter how brilliant a list of names of associates Lotsy reports, to join now an association which is directly opposed to the Centralblatt. . . .

I have written this to you not knowing to what extent you feel confidence in Lotsy as a manager but merely wishing to let you know the circumstances which prevent my accepting Lotsy's invitation. I feel that we here are in duty bound to give the Centralblatt a fair trial under the proposed arrangement. It may or may not succeed but, if it does not succeed now, it will be in part our own fault. If it turns out that the Centralblatt people won't do as they should, then all the subscriptions will go to Lotsy's journal if it is a good one. I take it for granted that the Centralblatt people mean to act in good faith. Certainly they already have dropped original articles from the Centralblatt itself, and, as far as American editors are concerned there has not yet been time for us to select editors and, until that is done, one could hardly expect a change in the character of American reviews. As far as improvement in the quality of the notices is concerned, I should expect that it would be gradual not sudden and we cannot complain if it takes at least six months before a perceptible change is seen.

Yours, truly,

W. G. Farlow.

Cambridge, March 1, 1901.

Dear Trelease,

This noon I received a letter from Lotsy which quite took away my breath. After receiving my cable and letter saying that it was too late for me to accept his invitation to join his International Society since we had promised to help the Centralblatt, Lotsy decided *to buy the Centralblatt*. They have agreed to sell out to him and he now wants me to cable to him at once that I will join the new Society. I shall take the liberty of thinking over the matter until tomorrow morning. I have consulted Thaxter and he agrees with me in thinking that I can and should accept. The way things have turned out is, I think the best solution of the complication.

[4] For a biographical account of Lotsy *vide* Genetica **13**: 1. 1932.

Uhlworm [5] is to be kept for five years and the American editors we may select will be accepted by the managers. It is proposed to capitalize the Centralblatt at $10,000. Stock is to be placed on the botanical market and we shall have the opportunity to invest. The assets on hand are something like 100 complete sets of the Centralblatt &c. The whole thing seems very funny but I think it will succeed. The main point as far as we are concerned is that our Committee is practically tied up for the present awaiting the cons[u]mmation of the purchase by Lotsy. There is one snag in the way; the purchase must be confirmed by the board of the new Society. What the board is I do not know but I suppose that Lotsy's action will be endorsed.

It is too dark to see what I am writing and I must close.

Yours, truly,

[W. G. Farlow.]

Cambridge, April 5 1901.

Dear Prof. Bower,

Since I wrote to you in regard to my correspondence with Dr. Lotsy I have had another communication from him which surprised me even more than his previous letters. A week ago I had a letter in answer to my inquiry as to the individual responsibility of stockholders in which he said but little about the Centralblatt but instead wrote several pages describing a project of forming a sort of museum for collections of microscopical slides and type-specimens in general and a bureau for collecting all sorts of miscellaneous information for botanical travellers.

I presume that you have received a similar letter. The letter seemed so very visionary and reckless that I could not help coming to the conclusion that Lotsy is what we call a "crank." He certainly must have very little commonsense. To manage the Centralblatt so that it would become a first-class journal will be quite enough to absorb all his time and energy without launching out on any such wild-cat sort of speculation as his proposed museum. He stated that he needed an endowment of $130,000. He added that Goebel despaired of being able to raise that sum in Europe. I am not surprised that Goebel thinks so.

Lotsy however is confident that some of the numerous American millionaires will give him all he wants. He said that if I thought that I could introduce him to a number of our millionaires, he would start for America at once and come to Cambridge to see me. Unfortunately I do not know any millionaires and if I did, I most certainly would squeeze all I could out of them for our own botanical department which needs money very much indeed and I should never think of asking them to give anything to Lotsy for a museum. I feel sure that all our botanists who are connected with universities or other institutions would act just as I have said I would do. There is only one rich botanist in this country and he certainly would give nothing to Lotsy in any case.

I have not yet answered Lotsy's letter because he said that he was going to send me at once the proof of the circular to be issued in regard to his new Society. After I have seen the proof of his circular I shall be able to write to him something definite.

[5] Shortly after Lotsy's Association acquired the *Botanisches Centralblatt* he and Uhlworm quarreled. After this Uhlworm retired entirely. He may have had some part in abortive attempts, made in Berlin a little later, to start another abstracting journal. These plans, however, did not materialize (*cf.* letter of Thost to von Schrenk, 15 Jan. 1902, in Chron. Bot. Archives).

He will not be able to see me in Cambridge because I intend sailing for Europe on April 30. My plan of going to Europe is due to the fact that I have been asked to attend the celebration of the University of Glasgow in June as a delegate of the National Academy of Sciences. I am so busy that it will be difficult for me to leave then but there [are] certain matters of business for me to attend to in Europe and my wife has two nieces of whom she is guardian now studying in Paris and she wishes to go to see how they are getting along. Unless something happens to prevent, we shall start for Cherbourg on the 30 and after attending to my affairs on the Continent, we shall go to England and then to Scotland so as to reach Glasgow in time for the meeting on June 15. Therefore there is a chance that I may have the pleasure of seeing you in June.

Yours, truly,

W. G. Farlow.

Between January 1902 and the last year of the First World War, Lotsy edited the *Botanisches Centralblatt* with great care. Except for Schramm's *Botanical Abstracts* we have never had anything in our field that was as satisfactory as the *Botanisches Centralblatt* between 1902 and 1919. Without doubt it was more satisfactory in its early years than later when Lotsy became involved in many other activities. During this entire period, Dr. William Trelease acted as American editor ("Président du Comité National des Etats Unis"). He gave freely of his time and energy and has done more than any other North American botanist to report well in the *Botanisches Centralblatt*, then still the only abstracting journal in our field, on the dramatic progress made in the U. S. A. between 1900 and 1918. He had the coöperation of a small, rather constant group of American "Rédacteurs spéciaux." Of some of these men the names do not often occur in the columns of the *Centralblatt*. That the journal was not always too satisfactory from the point of view of the North American editors and their colleagues we may gather from the following two letters of Dr. Farlow:—

Cambridge, Mass.,
Jan. 27, 1908.

Dear Trelease,

I see no reason why your letter should not be sent to Lotsy except that — suppose that he lets you go? Then I do not envy you if you feel that you *must* go ahead and start an American journal. I understand your reference to the Americans as "prepared to start an abstract journal of their own when etc" to be a bluff rather than a statement of fact. My recollection is that when I tried to suggest a journal for giving abstracts of American literature I was very, very coldly received. Galloway I remember as particularly "pooh-poohy" and declared it impossible. In fact, I do not remember that anyone gave me any real encouragement. Perhaps you mean an American journal giving abstracts also of European papers. Ughm!

I had no confidence in any real improvement in the Centralblatt when I heard that Lotsy was to run it . . . He is impractical and given to putting all sorts of unnecessary irons on the fire. Still I suppose it was a question whether any better man could be got.

Although far from what one would like, it may perhaps be the fact that, as at

present conducted, it is as good as such a journal is *likely* to be. Some want notices like those in Engler's Jahrb. but that is impossible unless few things are to be noticed. The "sympathetic" notices as I have heard them called can only come from experts who have taken their time to read and use the books noticed and that takes a long time and experts are not to be depended on in a journal like the Centralblatt. People complain of Just [Botanischer Jahresbericht] that it is too far behindhand. For my part, I think that Just is as near perfection as anything of the sort can be.

I was sorry not to go to Chicago but I was too dreadfully busy and I was especially sorry that none of our other botanists could go but Goodale was not well enough and Robinson, Fernald & Thaxter were driven by the printer and Jeffrey had only that time for going after fossil material. Like us, I suppose that you have had a remarkable winter. I do not object to the mildness.

W. G. Farlow.

Cambridge, Mass.,
Feb. 10, 1908.

Dear Trelease,

While you are "jogging" the Centralblatt let me suggest one point where it seems to me that something should be done. I do not like the idea of giving notices in the Neue Litteratur. Why should not the notices be given all of them in the regular place? It looks as if the reviewers despairing of getting their notices into the regular pages smuggled them in in the Litteratur where no one would look for them. It is unjust to writers so noticed since it looks as if their papers were not worth noticing.

Generally for my purpose the very short abstracts in the Neue Litteratur are enough and that brings up the question, Why not give *all* the notices in this short form with the titles issued in Neue Litteratur? Who wants (see p. 24) more than a page of *Dittrich* & *Pax*, Herbarium cecidiologicum? The trouble is and probably always must be that a good many reviews are given to immature students who have no sense of proportion.

Yours, very truly,
W. G. Farlow

The *Botanisches Centralblatt* was first published for the Association by Messrs. Brill of Leyden and printed in Germany at the same press where it had been printed before it was acquired by the Association. Later it was printed in Leyden at Sijthoff's and published by Messrs. Fischer of Jena for the Association. Though the Association remained a purely international society till the end (in 1917 its council was formed by D. H. Scott, Wm. Trelease, and J. P. Lotsy, and its editorial board by Wm. Trelease, C. Bonaventura, A. D. Cotton, C. Wehmer, and C. H. Ostenfeld) and though its journal was edited and printed during the First World War in a neutral country, the fact that it was published in Germany and that much (without doubt much too much for an international journal) of the contents was in German caused highly complicated difficulties with American (not British!) authorities and resulted in only a few copies of the journal reaching American subscribers. This and other things caused considerable ill-will amongst a number of North American botanists. In Great Britain, where the international character of the journal was better understood, as Drs. Bower,

Seward and others have assured me, there never was any feeling against the journal or the society. Feeling dissatisfied, a group of U. S. colleagues, amongst whom Dr. Livingston was very influential, established at that time *Botanical Abstracts*.

This probably happened too recently to allow us to decide if it was wise to do so, especially as Dr. Lotsy, tired of the difficulties of keeping his journal going during the war years, offered the North American group the fullest possible coöperation of himself and the Association, a somewhat informal offer in which he would, without doubt, have had the backing of the Council. This offer was not accepted. It was hardly considered, which seems a great pity to the student of international affairs. As a result, we have two not entirely successful [6] abstracting journals from 1919 onwards.

Since this article is not primarily concerned with these problems, it may be more interesting for us to see what Dr. Farlow, then nearly seventy-five, had to say about these developments in 1917, 1918 and 1919 in his letters to Dr. Trelease, the chief American editor of the abstracting journal of the Association Internationale des Botanistes till its discontinuation.

Cambridge
Nov. 12, 1917.

Dear Trelease,

I agree with you entirely about the desirability of improving the Bot. Centralblatt rather than starting a new and similar journal in this country.

Reddick was here a fortnight ago and asked my opinion about an American Centralblatt to include everything in this world and apparently everything in the Solar System. I do not think that he favored it. His account of the genesis of the idea of an American Centralblatt was not altogether clear. He said that Stevens had been impressed by the success of the new American journal of bacterial diseases to which he said there were a good many medical men who subscribed. The universal success of that journal did not impress me because I had never seen a copy and had not heard anyone speak about it. If, however, as Newcombe appears to desire, the new journal is to include not only reviews of bacterial papers but also all papers on plant diseases and even botanical papers on non-bacterial or pathological subjects, I have an idea that the medical subscribers would soon drop out.

My first answer to Reddick was that it seemed to me to be folly to start a new journal of any kind at this time for financial reasons. Until the war is ended no one knows whether he has any money to spend or not and it is now evident that the war will not end for a long time if it is to end in favor of the Allies and, if not, Germany will take all the money that can be squeezed out of us.

Reddick said that it was expected (?) that the Brooklyn Bot. Gard. would furnish the money or most of it. My answer to that was that if Brooklyn furnished the money they would have to do with it what the Brooklyn people wanted and that the desires of those who did not furnish any money would not count for much.

[6] It is not the fact that they were commercially not too successful which I deplore, but the fact that none of them was truly international, while in 1919 we did have the framework of a truly international botanical society! *Cf.* also my remarks in Nature **154:** 595. 1944.

Furthermore, I told him that I did not desire to see established a new journal of reviews. As it is, I am loaded down with journals which give reviews of the same papers over and over again and unless the current journals would agree to give up reviews or notices of new papers I did not want to be bothered with more reviews. He said that he believed that Mycologia would agree to give up reviews. Mycologia, however, is practically the organ of the N. Y. Bot. Gard. and has no interest in noticing any work not done in connection with the Garden. Other journals probably would not agree to give up reviews as they help to sell the journals. Those who take only one journal would be likely to select that one which gave reviews in addition to original papers. The notices in the Bot. Gaz. for example certainly draw in subscribers.

Again I have little faith in the ability of our enthusiastic friend to produce a journal which would replace the Bot. Centralblatt. As you say, at the present time that journal does not help us as we do not receive the current numbers but after the war I presume that it will reach us as before. If the Centralblatt gives out later, then it will be soon enough to think about an American journal.

The Bot. Centralblatt certainly might be made better. I hope that it will. I have my own ideas of what is desirable. In the first place no author should be allowed to write a notice of his own papers unless he happens to write in a language like Japanese, or Czech perhaps, which is not readable by the ordinary editors. In the next place the editors should not be allowed to turn over papers to their students to review unless they themselves revise the reviews themselves. The younger students are not capable of writing good condensed reviews and, being paid by the page, there is no inducement to be brief.

My own opinion, not shared by some, is that in a Centralblatt, as far as possible all papers should be noticed as soon as possible after their issue and that the reviews should be *descriptive* not *critical*. That is, one should have the titles and a brief statement of contents of as many papers as possible so that everyone can tell whether a paper on subjects in which he is interested is one which he ought to look at and read. Really critical reviews are generally too long for a Centralblatt, they must be written by experts and they cannot be written at short notice.

Robinson does not agree with me. He thinks that reviews should be "sympathetic." That means I suppose in a review of a systematic paper there should be a list of at least the new species and perhaps others with comments. Those not interested in the subject would I think have little "sympathy" with reviews of that kind.

I really do not believe that there are any botanists to speak of who really want a new journal but at botanical meetings the crowd is easily persuaded to vote for anything proposed by a pleasing and plausible speaker. If the question comes up at Pittsburgh (I shall not be there) it may be voted to start the journal. Some very foolish things have been voted by the A. A. A. S.

The war news is certainly depressing and people here see no end of the war in sight unless it is to be very unfavorable. Our family have lately been rather anxious. My niece we hear has reached France. Her brother, an aviator, is on his way somewhere, we do not know where. Our available relatives are now all in Europe except one who is still at the Hydroplane school in Pensacola.

Remember me kindly to Mrs. Trelease.

Yours, very truly,

W. G. Farlow.

Cambridge
Febr. 26, 1918.

Dear Trelease,

Do you remember the *Guepinia lutea* Bres. which you collected in Alaska? I have recently received from near Seattle a Guepinia near *G. merulina* which may be your plant. We have here what I take to be *G. merulina* but it is rather rare. The Seattle fungus has spores which are rather larger and more septate than in our plant. The basidia are narrow and the tips are very short . . . but I do not know whether this is constant. I had only a very little of the Guepinia with a large junk of the host. I have written for more and, if I get it, I will send you some for comparison if you have any of the *G. lutea* on hand.

I am just sending in my preliminary ballot of the Nat. Acad. I put on the names of Atkinson and Forbes. The latter will probably be elected. About Atkinson I am more doubtful for there are some who are strongly opposed to him. At any rate I do not see how I can go to Washington in April to the meeting. It appears now to be practically impossible to secure a room at any hotel and the journey is subject to long delays. Prof. Palmer a short time ago was 27 hours on the road to New York. To be sure he was so imprudent as to take the boat on a very cold day and was blocked by ice.

Reddick was here two days ago and told me about the Pittsburgh meeting. O. L. Howard was also here apparently to make arrangements about the Ass. meeting in Boston next winter. He is down on Richards for advising giving up the last meeting. If next winter is anything like this and no more coal than now, the A. A. A. S. will not be welcome guests.

Both Reddick and Howard said that the Pittsburgh meeting was a *great* success. Reddick had become a convert to the new extract journal and he said that they were going to start work at once and get out the first number in June. It appears that the Waverly Co. of Baltimore which issues the bacterial extract journal thinks it can make a success of the new journal. It is supposed that the larger libraries would be willing to pay a good price if they receive slips during the year and the whole in book form at the end of the year with the right to buy other copies at a reduced rate. Something was said of charging $10 a year to individuals. I thought that not many botanists would want to pay that amount but Reddick thought they would if the journal was indispensable. The *if* seems to be important but also uncertain. The Brooklyn Garden was said also to be ready to help. I was told that it was Macdougal who was negotiating with the Waverly Press.

I wonder what you wrote on your card of the Man Power Census sent by Kern I presume. Not knowing what else to write I wrote "Mycology" diagonally across the card but Thaxter says that that will only show what a fool I am for only a short time ago a prominent Phytopathologist said that he thanked God that he was not a mycologist. A letter has just come from Whetzel asking me to attend a meeting in Boston on March 1st to organize work. I can hardly suppose that they want me to help them do the talking for they are already well equipped in that department.

Winter seems to be gradually slipping away. That is we now have some comfortable days as well as some very uncomfortable and March is still to come.

Yours, very truly,
W. G. Farlow.

Cambridge,
Dec. 27, 1918.

Dear Trelease,

Things are in a mess. I presume that the fact is that the Centralblatt management has become unpopular on account of Lotsy's . . . domineering and not conciliating [manners].[7] Chodat I presume represents the attitude of some Europeans outside Germany and they would be glad to see a rival established in some other country. Most Americans, especially the young men, seem determined to cut out the Centralblatt as too decidedly under German control. . . .

If Botanical Abstracts seems to fill the bill it will have a circulation abroad as well as here. My question is, will it fill the bill? The first number was not reassuring but later numbers may be better. Livingston's statement that botanists were much pleased with it would have more weight if I knew who the botanists were who liked the first number. . . .

At any rate, the war is now on. The Bot. Abstracts committee after Livingston's letter do not desire to conceal the fact that they propose to squelch the Centralblatt if they can. If I were you, I would wait until getting an answer to your letters of Dec. 24 to Livingston and Williams & Wilkins. They will have to say whether they intended to have their letters sent to the Ass. Intern. or not. Very likely you already have their answers. If they did intend to have their letters, or their contents, sent, then of course they should be sent. If they say no, all you can do is to tell Lotsy that you have communicated with Williams & Wilkins and that they have no intention of explaining or apologizing.

As to your own relations to the Ass. Int. Bot., if you have reason to suppose that its affairs are run by "secret diplomacy" as Chodat says or, if you have reason later to think so, you had better withdraw. Otherwise you will be in the position of endorsing measures which you know nothing about and may not approve at all.

Whether the abstracts will win the day is after all to a great extent a question of dollars and cents. Just now it is a matter of sentiment but in the end the question will be can we in America compete with the Germans in cost of printing and can we control at as small cost as the Germans a large body of reviewers, revisers, etc. No one knows what the condition of scientific workers is to be in Germany. It may be that the hitherto large number of trained men willing to spend their time in writing reviews, bibliography and what may be called dictionary work at a very small price will be much diminished. I am skeptical as to the reduced condition of Germany. I believe that they are still in good condition and really better off than France and perhaps as well off as England in equipment for scientific work. Of course much depends on what sort of a new government is formed in Berlin. So far at least the German leopard shows no signs of changing his spots.

Yours, very truly,
W. G. Farlow.

[7] Lotsy, his work and his personality, has often been criticized. He was, however, a great man, and has done more than anyone in the history of our *scientia amabilis* to promote international relations, coöperation, and understanding. Dr. Farlow's impression, without doubt shared by others at that time in the U. S. A. (but not in Great Britain, where Lotsy had more friends than anywhere else), of Lotsy's political sympathies is so erroneous that one can hardly understand it. It teaches us how easily misunderstandings may arise, during years of mental stress, even amongst the representatives of a single race.

Cambridge,
Dec. 31, 1918.

Dear Trelease,

A letter from Mangin dated Dec. 13 contains the following which will interest you.

"Nous avons eu à discuter la question de l'Association Internationale des Botanistes. Cette association est virtuellement dissoute et les membres ne (parviendront . . . word not clear but this as far as I can make out) la liquidation. Mais il serait utile de la reconstituer entre alliés et d'utiliser comme organe de cette association les abstraits botaniques qui ont fait leur apparition chez vous."

If I understand the above, the association is bankrupt which can hardly fail to be the case since subscriptions must have fallen off very much. I am a loser to the extent of $500, the amount of bonds I took. . . .

It seems to me just as well that the Centralblatt should stop. I should prefer an international reorganization to a purely American conducted journal but Bot. Abstracts has started in ahead. . . .

Happy New Year,
W. G. Farlow.

Cambridge
Jan. 26, 1919

Dear Trelease,

It is to be regretted that Mangin's letter to me and Flahault's letter to Duggar did not reach us before the Baltimore meeting. Had that been the case it would have been possible for you to state the facts contained in those letters to the representatives of Botanical Abstracts at Baltimore and you might (?) have been able to convince them of the desirability of trying to make some international combination which would result in making the Abstracts really an international organ.

Now, the case seems to be rather hopeless. It looks as if our younger botanists were cock-sure that our American botanists were better able than any others to issue an international journal. I hear that a contract has been made with Williams & Wilkins to run the Abstracts for five years. Of course W. & W. are not well informed as to the requirements of a real international journal and are quite satisfied that the Abstracts, as illustrated by the single number issued is just what is wanted.

The number issued did not strike me favorably considered as a general international review of botanical literature. It was really nothing but a guide to American botanical literature and the value of notices of papers in Bot. Mag., Torr. Bull., Phytopathology, Am. Journ. Bot. does not seem to me to be great since all our botanists practically see all those journals for themselves. Furthermore the comparatively large amount of space taken by Shull to review papers having no relation to botany is looked upon unfavorably by all botanists I know and, unless he is to be restricted in later numbers, the journal will suffer.

Flahault's letter is interesting. He speaks of the *possibility* of the non-germanic members of the Assoc. Intern. breaking up the agreement with Fischer and apparently is not sure whether it can be done. Of course it can be done for in these times international pre-war agreements count for nothing.

It looks to me as if the feeling against the Germans is too great at the present time to make it possible to form any international union. Opinions may be modified later but not for some time to come. Meanwhile the Bot. Abstracts is to be pushed in the hope that it will replace the Centralblatt. If it is to succeed in that object, future numbers will have to be very different from the first number.

There is no doubt, as you say, that an abstract journal with a German title would not be taken in the allied countries now. But any journal would have to give abstracts of German botanical papers as well as others. Such reviews need not be in the German language although I see no reason why they might not. There are certainly more botanists here who read German than who read Italian. It may be said, however, that one can often "guess" what the Italian means.

The meeting [10] in London referred to by Flahault appears to be merely the general meeting of scientific delegates and it could not have had any special significance for botanists. I should be glad to know what Scott's opinion is.

In short, the Centralblatt is dead and cannot be revived as a world journal under a German title and issued by a German house. . . .

From the point of view of science itself, I do not think that a union of English, French, Italians and Americans alone would be for the general advantage. There therefore remains the question, what are the chances that Botanical Abstracts will fill the place of the Centralblatt? That cannot be even guessed now.

I hear that the editors of Bot. Abstr. wish to raise a fund of $1000 to pay reviewers. That would not go very far and the Brooklyn Garden people or others specially interested ought to be able to get that amount without a general appeal. If their credit is no greater than that, the future is not promising. . . .

W. G. Farlow.

Though Farlow's interests were wide, ranging from the lowest to the highest of the cryptogamic plants, from general botany to taxonomy and phytopathology, from music to the humanities, though he travelled very widely and intensively in the interval between 1872 and 1874, and he also visited Europe thereafter, he was not in the broadest sense an internationalist, as were some of his contemporaries. His own collections came first and these he protected for future botanists by adopting measures, often criticized by his colleagues, that would as far as possible assure that the collection of books and specimens be available to future botanists. However, despite this preference for his own collections, he was much interested in the personality of his foreign colleagues, as Setchell (Mem. Nat. Acad. Sci. **21:** 14. 1927) has told us, but as a practical and typical New Englander, was still more interested in the results of their work and especially in the abstracting and listing of the botanical literature on a truly international basis. To this extent he was an internationalist, and as his early letters show, a pioneer in American abstracting work.

CHRONICA BOTANICA
WALTHAM, MASS.

[10] One of the early meetings of the *Conseil International des Recherches* (now *International Council of Scientific Unions*).

PLATE I

2(1): 84–95 FARLOWIA January, 1945

DR. FARLOW'S INFLUENCE ON MYCOLOGY

William H. Weston, Jr.

In attempting to evaluate the imprint on a field of science made by an outstanding worker in that field it soon becomes obvious that lists of publications often tell but part of the story and that a man's influence may make itself felt through various channels less obviously recorded. In Dr. Farlow's case, even the partial list of his publications in two of the biographical memoirs noted in the introduction to this number, reveals that the scope of his activities was very broad including work on algae, bacteria, fungi, lichens, liverworts, mosses, ferns, and flowering plants. In addition he participated actively in botanical affairs for more than forty years, stimulated and promoted the development of several fields which were part of the rapid progress of botany in the United States during that time and played an active and constructive part in maintaining effective working relationships between botanists in this country and in Europe.

Withal it was probably the field of mycology which he influenced most strongly. On attempting to analyze his effect on this branch of botany, it becomes clear that his was a manifold influence making itself felt to some extent through his teaching, to some extent through his published work, but chiefly through his devoting to the service of mycology his wide knowledge of the fungi and his extensive herbarium, library, and indices on which this knowledge in part was based. The force which gave especial strength to all these lines of influence was the power of his dynamic, stimulating, and catalytic personality. His influence on mycology was thus of the same pattern as his much wider influence on the whole field of cryptogamic botany.

To understand the conditions which shaped his early aspirations into definite objectives that permanently motivated his life's work it is necessary to appreciate the status of botany in the United States during the formative years when Dr. Farlow began his training.

During the 1860s, botany as a science numbered very few professional devotees in its ranks in this country and was largely the avocation of those, such as ministers and doctors, earning their living in other professions. As an indication of conditions at that time it is worthy of note that Asa Gray, Goodale, Farlow and others in this country, like De Bary and numerous botanists in Europe, on graduation secured medical degrees because this was the only available degree involving training and graduate work applicable to botany and also because the profession of medicine would yield a

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Doctor and Mrs. Farlow on the porch of their summer home in Chocorua, New Hampshire. Picture taken a year or two before his death.

living permitting work in the unremunerative and precarious field of botany. Also, botany in the United States consisted almost wholly of work on the flowering plants with emphasis on their taxonomy, nomenclature, floristics, and gross anatomy. The leading exponent and symbol of this type of work was Asa Gray who from his contact with the botanists of Europe was well aware, upon his return from abroad in 1870, that rapid expansion of botany was beginning and if the United States was to keep pace, other aspects of the science should be developed. Accordingly, as he was fully occupied with his own field of work in the Fisher professorship of Natural History which he had held since 1842, he was instrumental in the selection and appointment of two promising young men well fitted to develop important fields in the rapidly expanding realm of botany.

The first, George Lincoln Goodale, by his skill as a teacher and lecturer gave impetus to the growing field of plant physiology which was rapidly assuming importance in Europe, and by his efficiency as an organizer and administrator built up the Botanic Garden, which already had more than half a century of development and service to its credit, to its final perfection of usefulness and beauty; he vitalized and enriched the collections of the Botanical Museum started by Gray around 1860 and brought about the building of the spacious and well-equipped Botanical Section of the University Museum.

The second, William Gilson Farlow, as the man to devote himself to the development of cryptogamic botany, was a wholly logical choice, for even before graduating in 1866 he had shown unusual interest and promise in that newly burgeoning field while his further preparation, through attaining his medical degree in 1870, was the same as that which Gray and De Bary, the two who influenced him most strongly, had themselves received. After being appointed Gray's assistant in 1870, Dr. Farlow worked with him for two years increasing the teaching emphasis on the lower plants, and carrying on research of his own, especially in the algae. Then, realizing the need for further training which only the laboratories of Europe could offer at that time, Dr. Farlow spent the two years from the summer of 1872 to the summer of 1874 travelling through England, the Scandinavian countries, Russia, Germany, France, Italy, and Switzerland and working in various laboratories, especially at Strasbourg in Anton De Bary's laboratory, at that time the world's leading center of mycological activity.

These were highly important years in Dr. Farlow's development. He met distinguished botanists and visited and worked in their laboratories, establishing valuable contacts and friendships which he maintained through correspondence and occasional visits for many years. He became acquainted with the botanists of the period, both the recognized authorities of the time and the students working with them, who were to become the leading mycologists and algologists of their generation. He visited the famous herbaria and libraries and the convincing evidence of their indispensable function as working tools essential to fundamental botanical work gained through his own use of these treasures, started him on the discriminating

purchases of books, periodicals, exsiccati, and specimens that were the nucleus of the Farlow Herbarium and Library so important in the ensuing development of cryptogamic botany in this country. Also in these two years he became aware of the trends and currents in botanical work and began to appraise what was fundamental and enduring, what held promise for future development, and what was merely transient in its temporary popularity.

The evidence seems clear that Dr. Farlow returned from his two years in Europe with the firm conviction that the development of cryptogamic botany in the United States would be aided most effectively by three essential facilities. First, there would be needed a herbarium of authentic reference specimens to facilitate dependable identification of critical material. Second, accompanying this reference herbarium an extensive and complete reference library on the fungi and other lower plants would be an imperative supplement for adequately identifying material and establishing its relationships and significance. Third, supporting both these, there would have to be an index comprising all significant published references to such material. One gains the impression that this enduring conviction was not so much the result of his earlier training with Gray as it was the logical outcome of his own analysis, during these two years, of the fundamental basis of botanical supremacy in Europe. There he saw, beneath the fads and fashionable trends which were gathering those adherents who gravitated naturally to the most publicized aspects of botany, the sound foundation on which botanical progress in Europe had depended. He realized the interdependence of such fundamental aspects of the science as taxonomy, morphology, and physiology, and retained his sense of proportion throughout his work in De Bary's laboratory where, even in the 1870s, the development, reproduction, life histories, parasitism, and other activities of fungi were stressed and taxonomy was rather looked down upon as old fashioned, the systematists with their vasculi being significantly and disparagingly dubbed "Heusammler." There he worked on the morphology, development, and reproduction of fungi and other cryptogams and there he completed the work on the apogamous development of *Pteris*, work which, on publication in 1874 immediately attracted wide attention, a pioneer contribution to a field later developed by others to far reaching significance. His brief but vivid pictures of the atmosphere, activities, and personalities in De Bary's laboratories as well as in others, such as Bornet's, in which he worked for shorter periods are interesting sidelights on botanical history, enlivened by his characteristic humor and well worth seeking out in the bibliographies already noted.

In the biographical sketches which appeared after Dr. Farlow's death, there is detectable a tendency to apologize for his not too extensive list of published contributions. This is hard to understand for certainly his biographers were well aware that the major part of his activity had been the accumulation and working over of collections not as an objective in themselves but as an essential working tool applied to the service of cryptogamic

botany. After Dr. Farlow's death in 1919 his herbarium, when officially taken over by Harvard, comprised around 500,000 specimens and the greater proportion of these had been studied and worked over critically by Dr. Farlow himself. Many of them bore enlightening notations in his characteristic handwriting, evidence of their having been compared critically with exsiccati, type specimens, and other pertinent material and carefully studied in relation to the significant and authoritative literature. No one who has consulted his extensive herbarium can fail to realize that it had been worked and reworked by Dr. Farlow until it became a fundamental part of his knowledge, his first hand familiarity with this material and with the wealth of original work in the extensive library he accumulated being the basis of his impressively wide knowledge so valuable to many workers in this country and in Europe.

The list of his publications is ample evidence of the breadth of his interest and knowledge since it ranges through the cryptogams from the algae to the ferns, and in the fungi, which have the major emphasis, touches on all of the chief groups from the bacteria and Myxomycetes up through the Phycomycetes, Ascomycetes, and Fungi Imperfecti, to the Basidiomycetes. Here are found the several published lists comprising some of the many fungi studied and identified by him, fungi representative of many different localities, some collected by him and by his associates in the vicinity of Boston, in the White Mountains, and elsewhere in this country, others brought back from the arctic by the Peary expedition or by other expeditions from such regions as Alaska and Labrador, or the Galapagos and the Bahamas, or from Bolivia and elsewhere in South America. Here are pioneer contributions to the field of plant-pathology, the papers on the Potato Rot, American Grape Vine Mildew, Black Knot of Cherries, and Diseases of Orange and Olive Trees in California representing part of his work at the Bussey Institution under his appointment as Assistant Professor of Cryptogamic Botany at Harvard from 1874 to 1879. There are also numerous more general papers in this field discussing common diseases caused by fungi, diseases of fruit-bearing trees, fungous diseases important in Massachusetts in various years and injurious fungi of California, while forest pathology shows a few pioneer papers on diseases of forest trees and diseases of trees following mechanical injury, and there are numerous studies of rusts, smuts, mildews, blights, and other fungi pathogenic to crop plants. There is also a brief note, in 1881, on the identity of a fungus found growing in the human ear, one of the early contributions in this country to the field of medical mycology, destined for rapid development forty years later. Papers on the edibility and poisonous quality of mushrooms and on the diseases of mushrooms with notes on truffles and other rarities are evidence of his interest in these economic aspects of the fleshy fungi as well as in their identity, classification, and relationship. There are notes also on some of the more spectacular aspects of mycology such as the identity of the fungi stored up by squirrels, the fungous diseases of insects, the mimicry of fungi by insects, the famous ginger beer plant with its combination

of yeast and bacteria collaborating to produce a liquor more or less potable, and the nature of the spots of rose leaves thought to be a fungous disease but in reality merely the adherent sporangia explosively discharged from a mold growing on the mulch below, a phenomenon that still continues to mislead botanists of the present as it did those of Europe and the United States in 1886.

His publications offer convincing evidence that even though his interests were primarily in systematic botany in its broadest sense, yet he was never oblivious of the practical importance of various aspects of cryptogamic botany and when need arose, turned his capabilities to such problems with telling effect, producing pioneer work of significance in several such fields. In plant pathology his work stands with that of Burrill and other pioneers as the first in this country, the beginning of a rapid development that within two generations gave preëminence in this field to the United States. Similarly his study of the pollution of drinking water by fresh water algae was a classic, pioneer investigation paving the way for the later development by Moore, Kellerman, and others, of this field of applied algology.

Even more than by his publications, however, he contributed to the advancement of botany by the services his herbarium, library, and indices, as well as his own knowledge, stimulus, and counsel rendered to workers in many areas of that rapidly expanding field of science. Here was available authentic reference material unequalled by any similar assembly in this country, a source where under the authoritative and interpretive guidance of Dr. Farlow, the interested seeker could find the answer to difficult questions as to the nature, identity, and relationship of the lower plants. Although such service is hard to estimate in quantitative terms, yet it was probably of the greatest value to mycology for while the library and herbarium covered the whole field of cryptogamic botany, the herbarium comprised more fungi than all the other specimens of algae, lichens, and Bryophytes combined, while the two Indices were devoted to the fungi alone. These facilities served, in the case of potentially harmful fungi, as the fingerprint files and criminal records from which the identity and dangerous characteristics of poisonous, injurious, or disease producing fungi could be determined with certainty, and, in the case of fungi potentially valuable to science, industry, or agriculture, as the personnel roster from which their names, locations, and capabilities could be ascertained. Visiting scientists in increasing numbers sought this center, coming from laboratories and institutions from all parts of this country and even from Europe, while by correspondence and exchange or loan of specimens and other material the service rendered by Dr. Farlow and the herbarium was extended even more widely. The numerous published acknowledgments frequently occurring in the significant contributions to botany over a period of nearly fifty years eloquently witness the effectiveness of this aspect of his participation in the development of mycology.

Dr. Farlow's influence on cryptogamic botany and on its specialized branch of mycology began through his teaching in 1874 when on returning

from Europe he was appointed Assistant Professor of Cryptogamic Botany at Harvard University and commenced work at the Bussey Institution in Jamaica Plain. There, along with his investigations of plant diseases, he guided the research of advanced students, the first of whom was Byron D. Halsted, while two days a week he gave more elementary instruction in general cryptogamic botany to undergraduates in Cambridge and in the summer gave similar instruction in summer school. After five years of productive activity in this dual rôle, he relinquished his work at the Bussey and under his appointment as Professor of Cryptogamic Botany in 1879 devoted all his time to a full program of research, teaching, herbarium work, and other activities in Cambridge. In 1891 he secured the appointment of Thaxter, one of his most promising Ph.D.'s then working at the Connecticut Agricultural Experiment Station, to carry on the undergraduate teaching while he travelled and worked in Europe and on his return in 1892 resumed only the instruction of graduate students working for higher degrees and the guidance of other investigators coming to the laboratories from the United States and abroad to carry on advanced research under his direction. In 1896, to devote himself untrammeled to the steadily growing herbarium, library, and indices that had become ever more important and widely known, he relinquished to Dr. Thaxter even this advanced instruction and guidance although his helpful and stimulating influence on graduate students through occasional contacts continued until shortly before his death.

Thus through seventeen years of undergraduate courses and twenty-three of official graduate instruction his influence as a teacher made itself felt on a long list of men from students gaining chiefly general pre-medical background to advanced workers who became important figures in various fields of botany. Of the courses he gave it was the general survey of cryptogamic botany variously designated in successive catalogues that most effectively demonstrated his unusual ability as a teacher. This covered the structure, development, reproduction, relationships, classification, and importance of the lower plants, a stimulating and challenging course with characteristically enlightening and enlivening lectures by Dr. Farlow and with strong emphasis on the laboratory study of representative types especially of the algae and fungi. He was at his best in the laboratory where his practical and psychologically sound approach of leading on the student by patient but persistent questioning used this plant material as a means for developing the student's latent capabilities for accurate observation, effective interpretation and precise description. His methods of inducing the student by his own efforts to master the difficulties of interpreting three dimensional features of complicated solid structures through interpreting the two dimensional visual images seen through the microscope were highly effective. A modern teacher might wonder what sections of a block of pine wood might have to do with cryptogamic botany or with mycology but as applied by Dr. Farlow, the study of this complicated structure became a highly challenging and enlightening initiation and apprenticeship to the

student with any spark of initiative or resourcefulness. Some of Dr. Farlow's convictions on the basic philosophy of teaching, the importance of the student's seeking and obtaining his answers from the material itself through his own efforts, are well brought out in his few articles on teaching, especially in his paper in the Scientific Monthly of 1886. Here the interested reader will find a discerning portrayal of some of the "modern" methods of teaching biology with a keen analysis of the types of students he finds in his own classes today and no teacher, no matter what school of education may have given him his training, can fail to profit by reading this paper of fifty years ago.

The facilities for this undergraduate teaching would now be considered meager. The microscopes were the early type Leitz, the low power focussing achieved by means of adroitly twisting the barrel in its friction collar, the high power focussing accomplished by a very simple but effective lever mechanism. Crude though they seem to the modern student, these little microscopes are still capable of excellent work in the hands of a discriminating user and several of them, fifty years later, still in good working condition, have given excellent service on field trips to present members of the staff. As there was no botany building until 1890, his courses were held in various buildings, one being Boylston Hall which still survives after years of service as a chemical laboratory, as a library, and in other capacities despite occasional arsonous attempts of students to do away with its gray grimness. From Dr. Farlow's class records it can be assumed that it was even more cheerless then than now for entries, during the winter of 1879, in his characteristic handwriting, record: "January 7: Lecture on Fucus and Vaucheria. Laboratory too cold for work. January 8: Lecture on the Peronosporeae. Laboratory too cold for work," while on January 14 it is safe to assume that milder weather enabled the class to make up for lost time as there was no lecture and the laboratory successfully covered *Fucus*, *Cystopus*, and the Saprolegniineae. Also, in these records there is a suggestion of the locker trouble which still afflicts the biology teacher, for in June, 1878, after the class of ten (six seniors, two juniors, one Divinity School and one Lawrence Scientific student) had successfully completed Natural History 9, he notes with relief that the microscopes (borrowed from the Botanic Garden and the Bussey Institution) are all accounted for but a new key will have to be made to replace the one missing for locker 16, although fortunately key #9 can do double duty as with a little coaxing it can be used for locker 8 in place of its own key, now lost. Incidentally it is of interest to note that Alfred Worcester, who became a leading and dearly loved physician in the vicinity, had locker 3 in 1877–78, Wm. Trelease, later Director of the Missouri Botanic Garden, had locker 1 in 1880–81, while in 1881–82, Wm. Patten, later widely known for his theory of the evolution of higher vertebrates from the Ostracoderms had #4 and Roland Thaxter #1.

The general course in cryptogamic botany served primarily to give a foundation knowledge of the lower plants to premedical and other students

concentrating in biology, and it fulfilled this function most ably giving the students also the training in observation and interpretation already noted and stimulating them with the enthusiasm and ideals which were essential ingredients of Dr. Farlow's teaching. Some, however, such as Thaxter, found in this course and in the more advanced ones to which it led, the impetus in mycology as a life work, hence even this undergraduate teaching contributed in some measure to Dr. Farlow's influence on this field.

It is for his guidance and training of more advanced workers that Dr. Farlow was best known and therein lay the major influence of his teaching on mycology. Stimulating in his leadership, exacting in his requirements, unfailing in his extensive knowledge, he contributed notably to the development of the men who carried on advanced work under his direction and guidance. It is significant that the problems he suggested for these men were almost without exception in the fields of morphology, histology, cytology, development, or parasitism rather than taxonomy. This was because, from the breadth of his own experience, he realized that such work would afford them better training than premature attempts at taxonomic monographs of various groups. Also he realized that systematic botany in its broad requirements needed all the evidence available from the rapidly developing fields of morphology and physiology as these, in turn, were dependent on taxonomy. In the testimonial volume of letters and photographs given him on his seventieth birthday, there is an impressive list of some fifty-eight workers who had studied with him or benefited from his guidance in the Cryptogamic Laboratory between 1874 and 1914. Among them are some who had become outstanding figures in algology, others had contributed notably to zoölogy, medicine, bacteriology, or even more specialized fields while the greater number were those who had played an important part in developing mycology and plant pathology in this country. The names of these men are eloquent testimony to the influence of Dr. Farlow's teaching; here are the algologists Setchell and others, discussed more fully elsewhere in this number; here are the outstanding teachers such as Lyman, Butters, and Grover; the famous men of medicine such as Kelly; the bacteriologists such as Barber; the botanists who contributed notably to physiology, genetics, cytology, taxonomy or other branches, men such as Blakeslee, Duggar, Davis, Robinson, Peirce, and Oakes Ames; the plant pathologists and mycologists such as Clinton, Rorer, Johnston, Faull, Schrenk, Fink, Riddle, Burt, Jackson, and Thaxter.

All the aspects of Dr. Farlow's influence on mycology and indeed on the whole field of cryptogamic botany whether through his teaching, research, or herbarium activities derived power and enhanced effectiveness because they were quickened and vitalized by his personality. In him there was no dullness nor stolidity, rather he was dynamic, highly active, quick of mind and motion, congenial, witty, and stimulating. Any discussion in which he participated was activated through his participation, any meeting he attended was enlivened and enlightened by his presence, any scientific move-

ment in which he took part was given impetus. Even as a student he had shown the qualities of leadership and as his prestige grew through years of constructive activity it was the stimulus of his personality as well as the soundness of his judgment and the scope and accuracy of his knowledge that made him one of the leading figures in American botany.

In the biographical sketches which appeared after his death, especially in the excellent memoir by Setchell, are many revealing glimpses of Dr. Farlow's personality, while the spontaneity of his wit and the deftness of his humor enliven the pages of his contributions. Some of the many stories concerning him have already been written, numerous others are treasured in legend. A couple of these, apocryphal perhaps, are yet so characteristic that they may not be out of place here. For example, it is said that his method of informing Dr. Thaxter of his engagement to Lilian Horsford was to remark at the end of one of their daily discussions of laboratory affairs, "By the way, did you know that Miss Horsford is engaged?" and on Thaxter's astonished reply "No, to whom?" his answer was "To me, of course." Also, as an instance of his ability to deflate pomposity there is the story that once, when he was crouched, digging in a patch of shrubbery by the side of a road near Chocorua, a dowager driving by with all the pomp and circumstance of footman, driver, and prancing team of horses, imperiously stopped the equipage and called out "Little boy, who are you and what are you doing here," at which Dr. Farlow straightened up and with impressive dignity answered, "Madam, I am Dr. William Gilson Farlow, Professor of Cryptogamic Botany at Harvard University and I am collecting hypogaeous fungi," at which disconcerting and unexpected reply the woman is reported to have driven rapidly away.

Those of us who were fortunate enough to have had even a slight association with him as graduate students looked upon him with awe because of his attainments, and delight because of his whimsical humour. While I was in charge of the teaching laboratory as Dr. Thaxter's assistant, Dr. Farlow liked to use the large tables on free days for sorting specimens and his comments, while rapidly shuffling packets from one herbarium folder to another and critically looking over new accessions, were worth straining the ear to hear and such straining was necessary, for in his rapid sorting, both hands would be busy and he would frequently hold one or two packets between his teeth. Yet even when thus occupied he would carry on a running fire of comment on the specimens, with anecdotes of those who had collected them and reminiscences of his own collecting trips. It was on one such occasion that he spoke of collecting algae on the West Coast and drying his specimens in his hotel room using the iron bed, which he had turned upside down and festooned with cords, as an improvised drying rack. His imitation of the Chinese houseboy opening the door, taking in the situation with one glance, uttering a wailing "A-wah, A-wah" and retiring for a more favorable opportunity to tidy the room, was unforgettable.

In some of the biographical sketches his quick, light step is noted. This step was familiar to all of us in the laboratories, as we noted also his char-

acteristic habit of taking a couple of shorter, quicker steps just before coming to a threshold, then hurdling the threshold as it were and taking a few more quick steps before resuming his regular pace. For this characteristic entry I used to listen, for he had a way of trying out my reactions on material he was studying, seeking, no doubt, the opinion of the general public of less experience and lower I.Q. in addition to the expert opinion of Dr. Thaxter. On one occasion he brought in two slides, asked me to compare them and after watching me examine them carefully under high power asked my opinion. My reply, noting their general similarity but pointing out certain differences, seemed to please him highly and he rapidly departed with the slides remarking "Very interesting, some people seem to have thought they were the same." When his note in Science in 1912 appeared shortly after, I realized he had been showing me some of the critical material on which his transfer of the chestnut blight fungus from Diaporthe to Endothia was based.

It was typical of his geniality that he would invite all of us, graduate students and investigators in the laboratory, to his house for occasional dinners. Those were memorable evenings. Mrs. Farlow, gracious and kindly, presided at the table over a dinner excellent and sumptuous, in striking contrast to the meager Foxcroft Hall menu which the limited finances of most of us permitted. Then followed examination of priceless volumes in the small fire-proof room which projected toward what is now the Faculty Club; perhaps some music in which he showed himself a versatile and accomplished pianist; and certainly reminiscences and anecdotes of his own experience and of well-known mycologists and algologists here and abroad, unforgettable to us who were just beginning our mycological work. On one occasion there was discussion of ant lions with which, of all of us, only Owen Burger was familiar from his work in Florida and I remembered well Dr. Farlow's vivid demonstration, on the deep piled Oriental rug, of the proper posture to assume while calling "Doodle, doodle" to the ant lion in the right pitch to jar him with the vibrations so that he would emerge from his hiding place in the center of his effective sand trap. (Incidentally, some years later in the Philippines I found this technique successful with the ant lions under the eaves of my house at Los Baños even though the magic words are certainly not Tagalog!)

All of us working in the laboratory at that time received some measure of help in our research and large measure of inspiration in our lives from our contacts with Dr. Farlow and this was in 1911 to 1915, only a few years before his death and long after he had, officially, relinquished all direct contact with teaching. Still treasured in my collections are specimens he gave me to facilitate identification of algae I had gathered at Woods Hole and around Halifax, Nova Scotia; but one of many instances of his helpfulness toward all of us.

That his aid to the younger generations of botanists was not only scientific but in many cases financial is less well known, largely because such aid was very unobtrusively and often vicariously and anonymously achieved.

Setchell mentions one or two such instances and Mangin gratefully acknowledges other cases in which French botanists were benefited while many others have received only private, though equally grateful, recognition.

To the beginner as well as to the expert, the assistance of Dr. Farlow's incomparably broad and expert knowledge was ever available if sought, and as his prestige grew he occupied a position of esteem highly regarded in this country and abroad as the final authority in difficult questions in the taxonomy of the lower plants. Because of his wide contacts with the work going on in this country and Europe, his familiarity with a wider range of workers and laboratories, his grasp of important developments in various fields, his opinion was sought not only in taxonomic problems but also on questions of general policy and purpose. It was the combination of his accomplishments and his personality that made him the spokesman and representative of mycology for many years.

Finally, as an adjunct to all the other channels through which he influenced cryptogamic botany and especially mycology, the part he played in fostering and promoting publications should be noted. He was the first of the American editors of the influential British periodical *The Annals of Botany* and, through his efforts over several years, it was strengthened by notable contributions from workers in the United States. In 1883, as the volume of work accomplished by investigators in his laboratory began to increase, he started the well-known series of "Contributions from the Cryptogamic Laboratories of Harvard University" and in 1896 when he relinquished the guidance of research workers to devote himself to his other activities, the numbered publications in this series, together with a few similar contributions for one reason or another not included, had reached over forty. Each of these papers by students or associates was within its limits a significant contribution and the whole series has materially advanced the development of botany. This very periodical, *Farlowia*, itself is not only a tribute in name to Dr. Farlow, but concrete evidence of the ways in which his influence on mycology and other branches of cryptogamic botany still makes itself felt.

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THE BOLETINEAE OF FLORIDA WITH NOTES ON EXTRALIMITAL SPECIES

I. THE STROBILOMYCETACEAE

Rolf Singer

The present paper is intended to be the first of a series of monographic studies devoted to the basidiomycetous flora of the State of Florida. For the first time an attempt will be made to treat taxonomically the Boletineae of a large region with due consideration of the anatomical and chemical characters which, according to the author's view, are of no less importance in the taxonomy of this group than are adequate macroscopical descriptions. Considering the many inevitable changes that will be noticed when this paper is compared with the traditional American and European system of classification, the author has thought it necessary not to confine himself to the discussion of the species occurring in Florida. While giving complete descriptions only of Floridan species, notes on extralimital species have been added in order to avoid the mistake bound to impair the work of the locally minded taxonomist. At the same time, this procedure seems to offer the only means of providing all the evidence for the solutions proposed in this paper. This seemed especially important in some thus far controversial groups such as the Strobilomycetaceae, Jugasporaceae, and the genera *Pulveroboletus* and *Phylloporus* (Boletaceae). It will also illustrate the specific representation of Florida fungi in the world flora of Boletineae. Another factor determining the present arrangement was the author's wish to coordinate European and American nomenclature and classification. As a matter of fact, many so-called American species have been known previously to European mycologists under other names, and many European species have turned out to be later synonyms of species published by American authors. In the case of *Boletellus ananas* and *Strobilomyces pallescens* a similar situation has developed between this country and Australia.

The mycoflora of Florida is especially fascinating as a part of a transitional vegetation, changing from the North American temperate flora into the neotropic flora of the West Indies and South America. This transition is a very gradual one, the northern and the southern elements overlapping each other's areas of distribution until, in a comparatively narrow zone along the south tip of the peninsula it becomes more truly tropical. Nevertheless, while not all of the representatives of tropical South and Central America reach South Florida, there are still some more northern elements following the species of *Quercus* all through the peninsula. The extratropical part of what is usually called South Florida, and Central Florida up to a line running somewhere through North Florida is characterized by a progressive (towards the north) increase of the absolute number of species

of Boletineae observed, and also by the progressive increase of the percentage of northern elements, and a progressive division of the fungus flora into a thermophilous summer flora, and a more hardy winter flora. While all tropical elements reaching as far north as the zone where this bi-seasonal fruiting period becomes most distinct naturally belong to the summer crop, and all the truly northern elements belong to the flora appearing during the winter rainy period, it nevertheless is true that some unexpected fruiting periods are observed for certain species, e.g. *Boletellus Russellii* with the center of its area far to the north, still appears among the summer-fruiting species. The bi-seasonal division rather seems to follow generic lines: with very few exceptions the genera *Strobilomyces*, *Porphyrellus*, *Boletellus* (i.e. all Strobilomycetaceae), all Boletaceae except *Suillus*, and the genus *Clitopilus* belong to the summer-fruiting group, while *Gomphidius*, *Suillus*, *Paxillus*, and also some single exceptional species of the Boletaceae belong to the winter-fruiting group. These seasons are strictly parallel with the main periods of precipitation in this part of the state, i.e. June to August, and depending on yearly fluctuations, frequently also in May (even April) and September (October) and again in December and January (sometimes including November and February, or even a part of March). The periods between these rainy seasons are not altogether devoid of fungus growth, but only very exceptionally will any representatives of the Boletineae be found during the dry spells between the main seasons, and never will a typical summer species be collected in winter or vice versa. It will be remembered that a somewhat similar situation has been reported from Cuba by Berkeley and Curtis though their statement regards only the montane zone. A very interesting fact, noteworthy in this connection, is the discovery by the writer of true seasonal dimorphism in boletineous species in Florida. Two species, *Suillus hirtellus* (Peck) Snell and *Suillus australis* Sing. ined., show morphological and chemical differences according to the period of appearance and these are constant in different regions and analogous in both cases cited. They will be discussed in the second part of this paper.

As for the association of the mycorrhizal fungi with certain hosts, we have some interesting examples of specialization in Florida. While it is generally assumed that there is little evidence of a mycorrhizal association with forest trees in the tropics, the occurrence of several species of pines down to the south tip of the peninsula and in the Caribbean area, and also the presence of many species of *Quercus*, explain the fact that Florida, even the tropical zone of the State, makes an exception of this rule. It is remarkable that two closely allied species, or, in the writer's opinion, subspecies of the genus *Pinus*, i.e. *Pinus palustris* and *Pinus caribaea*, clearly differ biologically if their symbionts are considered. *P. palustris* has many more species forming mycorrhiza obligatorily confined to *Pinus*, and most stands and a fair number of individual trees if not practically all in certain regions have either *Boletinus*-, *Suillus*-, or *Gomphidius*-mycorrhiza, or at least a mycorrhizal flora consisting of such Agaricales that do not occur on or with *Pinus caribaea*. This latter pine is comparatively rarely found to be asso-

ciated with mycorrhiza-forming Agaricales, and if it is, the species are different from these observed with *Pinus palustris*. *Pinus caribaea* is associated with *Boletus griseus*, *Boletus rubricitrinus*, *Boletus rubellus* or in other words, fungi belonging to a genus where the formation of mycorrhiza with conifers is *not* a generic character, and not even a specific character as far as the species just cited are concerned. This strict differentiation may best be explained by the fact that to the slight morphological difference between these two pines there is added a somewhat stronger climatic, and a very strong ecologic factor. Most of the area of *Pinus caribaea* is situated in the tropical zone of South Florida, and the soil usually is hot and dry lime rock. Whatever the explanation, this interesting relationship tends to furnish one more argument in favor of the results of the recent investigations by De Vall.[1] For example, in Highlands Hammock State Park, *Pinus caribaea* was indicated by some authors. The analysis of the accompanying fungus flora shows that it is typical for *Pinus palustris*, not *P. caribaea*. Actually De Vall and West were able to state that the pines in question were *Pinus palustris*.

As for broad leaf trees the main mycorrhiza-bearing genus is *Quercus*. Here again I was surprised to learn how little some of the mycorrhizal fungi seem to be influenced by the variety of vegetation types in which they find themselves growing, as long as there is a suitable mycorrhiza-host. In Florida all species of *Leccinum* (group of *Boletus scaber*) are dryophilous, i.e. confined to oak; yet, *Leccinum chalybaeum* Sing. ined. was found in a flatwood under *Pinus palustris*. It was not until I found a patch of two inch high *Quercus minima* that I felt satisfied to explain the occurrence of a *Leccinum* in pine land. This little oak also grows in symbiosis with *Porphyrellus subflavidus*. Only a few feet from there, I observed several carpophores of *Tylopilus conicus*, an indubitable pine-symbiont. This shows how carefully field observations on questions of mycorrhiza must be made in order to provide information usable as a basis of future experimental studies or as a basis for taxonomic purposes, instead of being a source of perpetual confusion.

Among the vegetation types of Florida in which species of Boletineae participate, are all types of hammocks (low, mesophytic and high hammocks as well as tropical hammocks), pinelands (high pine and flatwood, and even the lime rock flats of the south) and scrub areas on white sand in sand dune regions. This enumeration is given in the order of relative importance. In open grassland (lawns, grassy places in open woods and gardens, so-called prairies and marshes, possibly some parts of the everglades), Boletineae are found exceptionally: *Clitopilus Giovanellae* and *C. scyphoides* var. *floridanus* being the only species growing outside the wooded areas, entirely independent of mycorrhiza, or as it seems even of the cooling shadow of the trees. In the scrub, under scrub oak and *Pinus*

[1] De Vall, Wilbur B. The taxonomic status of *Pinus caribaea* Mor. Proc. Fla. Acad. Sci. for 1940, **5**: 121–132. 1941.

clausa, I have observed two species of boletes, *Boletus luridellus* and *Xerocomus subilludens*, but both these species are not exclusively inhabitants of the scrub.

A phenomenon of interest for those who search for new and more convincing facts about the problem of the phylogeny of the Boletineae, has been observed in Florida: the occurrence of gastroid conditions of two now well-defined species of Boletineae, *Boletus rubellus* ssp. *caribaeus* and *Boletinus decipiens*. In the former they were sterile while in the latter they produced spores of the same kind as were produced on the pseudoangiocarpus (i.e. normal) carpophores. These parallel forms of fructification will be discussed at their proper place.

The number of species of Boletineae now known to occur in Florida is at least sixty-five. This is a remarkably high figure if the fact is kept in mind that several genera of prevalently or exclusively northern distribution such as *Gomphidius*, *Paxillus*, *Hygrophoropsis*, and *Boletinus* are represented by only one or two species each, and still the total is higher than in France, and probably higher than in the entire Caribbean area.

Some special arrangements and abbreviations to be used on the following pages need explanation. In the key, the Florida species are numbered, the numbers referring to the figures preceding the name and description of the species in this paper. The unnumbered species appearing in the keys are those studied by the author and discussed in this paper under the heading "Extralimital species." For these species the synonymy has been kept down to the most essential binomials, while for Florida species the full synonymy has been established. The keys are thought to enable the experienced reader to determine all sufficiently known species, and it is hoped that this arrangement will make the paper useful beyond the state limits of Florida, and also abroad. Under the heading "Material studied" we indicate all material that has influenced the description given. The herbaria in which the material is preserved are indicated by the abbreviations recommended by J. Lanjouw (Chron. Botan. **5** (2/3): 143–150. 1939). A special effort has been made to secure type material of the species treated, and all material collected in Florida, though the latter is scarce with the only exception of the collections preserved at the Agricultural Experiment Station of the University of Florida at Gainesville. All the descriptions and notes including the synonyms are entirely based on the author's own studies; in the very few cases where citations from other sources have been necessary, these sources have been indicated. A complete list of literature and an index of the genera and species will be given at the end of the last part of this paper which will be published at a later date. The color names in quotation marks given in the descriptions and keys are those of R. Ridgway. *Color Standards and Color Nomenclature*, Washington, D. C., 1912. Colors that cannot adequately be determined in Ridgway are cited from A. Maerz & M. Rea Paul, *Dictionary of Color*, New York, 1930, and then "M. & P." is added after the quotation marks.

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SUBORDER BOLETINEAE

(Opat. 1836 ut fam.) Pilát 1926, em. Sing. 1936.

KEY TO THE FAMILIES

A. Spores never pale pink, mostly ornamented, or nearly black to black, or "warm sepia" to "Sudan brown" or "hazel" in print, or extremely long (most of the spores of a print over 20 μ in length), or with thick (thicker than 1 μ) wall applanate or provided with a germinative pore at the apex; hymenophore ordinarily (in Florida always) tubulose; clamp connections none. *Strobilomycetaceae*

A. Spores either pale pink, or smooth (constantly and persistently so), if nearly or distinctly black, and if larger than 20 μ, the hymenophore is lamellate; if thick walled, the hymenophore usually lamellate and/or clamps present; germinative pore none; hymenophore either lamellate or tubulose, also merulioid, or showing some configuration intermediate between lamellae and tubes; clamp connections absent or present.

B. Hymenophore tubulose, rarely merulioid or (in one species) lamellate, and then the surface of the pileus turning deep blue or greenish blue with ammonia. *Boletaceae*

B. Hymenophore lamellate. Pileus not turning deep blue nor greenish blue with ammonia.

C. Spores evenly rounded, not angular when seen from one end, nor furrowed; carpophore usually well pigmented, mostly strongly reacting with NH_3, KOH, $FeSO_4$, and other reagents; lamellae sometimes very thick and distant; clamp connections absent or present; cystidia absent or present.

D. Clamp connections absent; spore print black or nearly so; spores clearly elongate. *Gomphidiaceae*

D. Clamp connections present; spore print brownish to "chamois," or creamy white; spores not elongate. *Paxillaceae*

C. Spores not evenly rounded but angular when seen from one end, and often furrowed longitudinally; pigment none, or grey, not reacting strongly with NH_3, KOH, $FeSO_4$; lamellae not thick, not distant; clamp connections none; cystidia none. *Jugasporaceae*

STROBILOMYCETACEAE Gilb. 1931 (as Strobilomyceteae)

Characters of the family: pileus squamose to squarrose, or squamulose to glabrous, viscid, or not viscid, small to large, the margin frequently projecting; hymenophore tubulose, rarely lamellate, tubes usually comparatively long halfway between the margin and the stipe, strongly convex beneath, depressed to adnate, pores medium wide to rather wide when mature, discolorous or more frequently concolorous with the tubes: whitish to gray, or whitish to pale grayish cream color, becoming vinaceous pink to sordid purplish, or yellow, golden yellow, the pores sometimes orange to red, at last olivaceous or yellowish brown, sometimes becoming black or red or blue on pressure; spore print black or nearly so, or, if not black, a very deep brown with an olive tinge, or deep porphyry brown to reddish brown ("warm sepia," "hazel," "Sudan brown"), rarely (in some olive brown colors) approaching spore colors observed in Boletaceae, and then the spores differing microscopically from the latter; spores richly colored when quite mature, fuscous to deep succineous-melleous with darker ornamentations, more rarely persistently smooth, and then either remarkable for their unique size (20–30 μ), or for their comparatively thick walls (more than 1 μ), the endosporium usually interrupted at the apex forming an incomplete germinative pore, or the apex applanate as in *Ganoderma*, or even more acuminate than it is in the Boletaceae, outline globose, subglobose, short-ellipsoid, fusoid, rarely ovoid-fusoid; basidia and cystidia comparatively voluminous, mostly more so than in species with equally large spores in the Boletaceae; trama of the tube walls pronouncedly bilateral in youth, hyphae without clamp connections; stipe usually approximately cylindrical and comparatively long and slender, smooth or ornamented with woolly or fibrillose scales, or with shallow to conspicuously projecting alveolate networks which in the most typical cases are waxy-slippery and bearing a hymenium; veil often present and then woolly or membranaceous, sometimes viscid, either fugacious, or persistent as an annulus on the stipe or as an appendiculate margin on the pileus, or both, never entirely glutinous, not yellow-pulverulent; stipe always destitute of glandulae. Habitat: on earth, more

rarely on the base of trees, or on decaying wood, most species preferring warmer climates, only few species reaching Canada, and only two species known from Europe and adjacent regions.

When any local flora is analyzed, and the representatives of the Strobilomycetaceae compared with each other and with the Boletaceae, it usually so happens that they are not easily recognized as a homogeneous group. In Europe, the spores of the two species found there are very different, especially when studied superficially, and the classification of the boletes according to the macroscopical characters of the pileus and the stipe that had been usual in both the American and European literature up to very recently, deepened the impression that the North American Strobilomycetaceae were widely different macroscopically, and only the "incidental" characters of the spores simulated an allegedly non-existing relationship. Even in South Carolina with its comparatively rich flora, Coker and Beers (The Boletaceae of North Carolina, p. 74. 1943) offer the opinion that "to attempt to segregate them as a unified group would seem to us unadvisable." Strangely enough, the strobilomycetaceous species of *Boletus sensu lato* in the same book are arranged much like a unified group. They are treated in the following order: *B. chrysenteron sensu* Coker & Beers, *B. sordidus*, *B. chrysenteroides*, *B. ananas*, *B. Russellii*, *B. betula*; — missing only *B. gracilis* (with the spores erroneously described as smooth), *B. projectellus* (with actually smooth spores), and *S. floccopus* (already transferred to *Strobilomyces*). If all characters and all species known (including those from Asia and Africa) are considered, there is no doubt that the Strobilomycetaceae constitute a natural group though related to the Boletaceae, but not showing clear kinship with any genus of the latter, differing not only in the finer structural characters of the spores, but also in their color. They also show a combination of macroscopical characters that makes it possible for the experienced collector to foretell the spore characters in a general way. The relatively large size of the basidia and cystidia in most species is an additional criterium. The astonishing similarity of the chemical characters in *Porphyrellus pseudoscaber* (Secr.) Sing. and *Strobilomyces floccopus* convinced the author still more that the two species and genera are more closely related than formerly has been suspected. Nevertheless the gaps between some of the species known to belong to the Strobilomycetaceae may seem rather wide, and some striking types may, rightly or wrongly, cause the taxonomist to erect several separate genera. That is why, indeed, a number of genera already have been proposed for them: *Strobilomyces* Berk. for *S. floccopus* and allied species, *Porphyrellus* for *B. pseudoscaber*, *Boletogaster* for a form near *B. jalapensis*, *Frostiella* for *B. Russellii* and *B. betula*, and *Boletellus* for *B. ananas*.

However, if all the groups and species from all over the world are taken into consideration, the gaps between them do not appear to be so striking, and one has difficulty in distributing the species in genera or sections rather than in finding connecting links between them.

In Gilbert's scheme the Strobilomycetaceae are divided in two genera

according to the color of the pores which are whitish to gray in *Strobilomyces* and yellow in *Boletellus*. In *Leccinum* where a similar situation exists, this character has certainly no more than sectional value, and neither in *Leccinum* nor in the Strobilomycetaceae is it accompanied by any one correlated character. As a matter of fact, the squamose condition of the pileus in *Strobilomyces* is found in the type species of *Boletellus* as well as in some other species and the reticulate short spores known in *S. floccopus* are also found in *Boletellus retisporus*, a species characterized by yellow pores. Thus, the characters on which Berkeley based his genus *Strobilomyces* are not confined to this genus. On the other hand two African species, *S. costatisporus* and *S. pterosporus*, have all the essential characters of *S. floccopus* but short ellipsoid spores with the same longitudinal wing-like ridges as known in *Boletellus singaporensis* or *B. Linderi* where the pores are yellow or red and the veil of a quite different structure.

Likewise the color of the spore print, often useful in the classification of the Boletaceae, cannot be used to the same extent in the Strobilomycetaceae because of the uncorrelated occurrence of some shades and tinges. The spores of *Strobilomyces floccopus* and *Boletellus ananas* are practically black in print though very different in ornamentation and shape. *Porphyrosporus subflavidus* and *Boletellus betula*, otherwise similar, have the hymenophore, including the pores, and the spore print differently colored. *Porphyrellus subflavidus* and *P. pseudoscaber* though having the same color of the spore print, are chemically different. The species, intermediate between these, *Porphyrellus gracilis*, has the spore print of a shade differing from both *P. subflavidus* and *P. pseudoscaber*.

The situation is not even as simple as it seemed at first, since further studies revealed that there are not merely white, gray, and yellow-pored species, as Gilbert thought, but there also are species with initially pallid cream-colored pores (e.g. *Porphyrellus tristis*), species with white pores that become vinaceous pink (*Porphyrellus gracilis*, *P. malaccensis*, *P. subflavidus*) and species with discolorous pores that are tinged orange to red (*Boletellus Linderi*, *B. pictiformis*).

The spores are not only of two types, as was assumed formerly, but there is a third type, usually described as punctate or warty; in this case cylindric spines are imbedded in the exosporium, protrude slightly or scarcely, and when they are seen from above, they are described as punctate (*Boletellus betula*), but when they are seen in an optical section showing the outline, the spores are mostly described as smooth (*Porphyrellus gracilis*, *P. subflavidus*), or warty-rough (*Porphyrellus malaccensis*). In some cases species with the essential macro- and microcharacters of a related rough-spored strobilomycetaceous species, have not seemingly but actually smooth spores. These smooth spores are easily recognized as strobilomycetaceous by their size, color, or thickness of the walls, or by the shape of the apex of the spore, and the combined remaining characters put these forms in the neighborhood of some indubitable representative of the Strobilomycetaceae rather than of the Boletaceae where most smooth-spored

species belong. If it could be doubted hitherto that there are any smooth-spored species actually related to some species of Strobilomycetaceae, there is no longer room for doubt in view of the fact that both smooth and perforated spores are found in a single preparation of some specimens of *Porphyrellus pseudoscaber* and *Strobilomyces velutipes*. In both cases the majority of the spores is smooth even when mature, and the minority is ornamented.

The above-cited facts show that the Strobilomycetaceae are a homogeneous and probably natural family within the Boletineae, and have to be treated as such. The difficulty in achieving a satisfactory division of the family into genera could be avoided by recognizing only one genus for all tube-bearing Strobilomycetaceae. However, this would mean only a delay in solving the problem, for poor genera will never constitute good sub-genera, and a single genus for all species involved would not match well with the generic conception now generally adopted in the Boletineae.

As the author thinks now, the difficulty will be solved most naturally by adopting a third genus for the tube-bearing Strobilomycetaceae, besides *Strobilomyces* and *Boletellus*. This genus has to take care of the species with elongate, smooth or perforated spores that are somehow reddish brown without an olivaceous tinge. Since it would therefore, among others, embrace the *Porphyrellus pseudoscaber*-complex on which the genus *Porphyrellus* has been based, it would not be necessary to propose a new generic name, but merely transfer *Porphyrellus* from the Boletaceae to the Strobilomycetaceae. Gilbert, not satisfied with the naming of a genus to replace Bataille's homonym *Phaeoporus*, transferred several species to *Porphyrellus*, American species which he did not know personally. Here started a confusion which could only be straightened out by studies in America using fresh spore prints and good fresh and authentic dried material. It turned out that *Boletus alboater* Schw. and *Boletus nigrellus* Peck, synonyms by the way, are not related to *Porphyrosporus pseudoscaber*, as claimed by Gilbert, but belong in a section of *Tylopilus* Karst. (Boletaceae), not represented in Europe. This explains why Gilbert, not knowing any species of this section, and not having recognized the genus *Tylopilus* at all, misplaced the two American species in his *Porphyrellus*. *Porphyrellus* differs from the above-mentioned section of *Tylopilus* in having more strongly pigmented spores with a different color in print, and with thicker walls and either acuminate or truncate apex. It also differs in having the pores much wider in age than they are in *Tylopilus*, and finally, their hymenial elements are decidedly more voluminous, especially in proportion to the spore size. The chemical reactions also support this distinction. Though both in *Tylopilus* and in *Porphyrellus* there are species with a pileus covered by a trichodermium, the structure of this layer on the stipe is not quite identical, and the trama of the dissepiments of the hymenophore becomes subregular in *Porphyrellus* in a much earlier stage than in *Tylopilus*. The occurrence of spores with imbedded warts in some forms of *Porphyrellus pseudoscaber*

emphasizes the difference between the type species of *Porphyrellus* and the genus *Tylopilus* with its constantly smooth spores.

Porphyrellus pseudoscaber is closer to *Strobilomyces* than it is to *Tylopilus*. This is shown by the occasional presence of spores with imbedded warts as in *Strobilomyces velutipes*, by the above-mentioned similarity of the chemical characters of *P. pseudoscaber* and *S. floccopus*, the intensely colored spore wall in both *Porphyrellus* and *Strobilomyces* and the rather large size of the basidia when compared with spore size in both these genera. It is therefore surprising to see that Fries, who did not know any of these characters, by mere intuition treated *Boletus porphyrosporus* (i.e. *Porphyrellus pseudoscaber*) and *Boletus strobilaceus* (i.e. *Strobilomyces floccopus*) within one and the same subgroup, the section *Favosi* Fr. Bataille had *Eriocorys strobilaceus* (*S. floccopus*) and *Phaeoporus porphyrosporus* (*P. pseudoscaber*) in the same series *Porphyrospori* Bat.

There are now about forty-five species known to belong or probably belonging in the Strobilomycetaceae, of which roughly two thirds are sufficiently studied. Only about one quarter of the total is not prevalently or exclusively tropical or subtropical. Eight species and varieties have been observed in Florida. Three of these are endemic and have not thus far been found except in Florida. This puts Florida among the regions richest in strobilomycetaceous forms: the southern part of North America, the West African tropics, and the Austral-Asiatic tropics.

KEY TO THE GENERA AND SECTIONS

A. Hymenophore, at least in its larger part, distinctly tubulose.

B. Spores globose to short-ellipsoid; hymenophore white to gray at first, becoming darker in age; stipe not waxy-costate-lacunose; pileus and stipe either warty, or woolly, or spinose-squarrose. Gen. *Strobilomyces* Berk.

C. Spores smooth, at least a considerable percentage of them, or beset with warts and short ribs, or either of these, imbedded in the exosporium, or with more or less imbedded to subfree reticulate ridges, forming a complete network on the spores. Sect. *Genuini* Sing.

C. Spores with longitudinal wing-like thin ridges running from the hilar end toward the very apex of the spore where they abruptly stop. Sect. *Pterospori* Sing.

B. Fungi not combining the characters outlined above, i.e. spores either distinctly elongate (Q larger than 1.5), or hymenophore not white, nor gray at first; if the pileus is adorned by projecting warts, floccons, or erect spines, the hymenophore is yellow to red, and the stipe is not strongly floccose, or a veil is entirely wanting, or both.

D. Tubes and pores of the young carpophores white to pale grayish cream color, becoming light pinkish vinaceous or sordid gray to porphyry brown with a grayish tinge when quite mature; spores with short cylindric warts or with slender spines perforating the exosporium, making the spores appear punctate when their upper surface is focussed upon under an oil immersion lens; in some cases the vast majority, or even all spores are virtually smooth; spore print reddish brown (varying from "warm sepia" to "Sudan brown" or "hazel") without any olivaceous tinge (at least in the species studied in this regard). Gen. *Porphyrellus* Gilb. em.

E. Pileus sordid or at least very dull-colored, gray or umber. Context strongly reacting with KOH, NH_4OH, $FeSO_4$, etc.; stipe not reticulate, its trichodermium usually forming a distinct velutinous layer.
Sect. *Tristes* Sing.

E. Pileus brighter-colored: yellowish brown, light yellow to occasionally white, chestnut, or some other shade of reddish brown; context either weakly or not at all reacting with KOH, NH_4OH, $FeSO_4$, etc.; stipe at least finely or even obscurely reticulate, or partly so, or deeply waxy-costate-lacunose. Sect. *Graciles* Sing.

D. Tubes yellow in youth, becoming melleous or brownish melleous, or olivaceous with age; spores with an ornamentation as described above, or smooth, or — most frequently — longitudinally winged to striately ribbed, in one species reticulate as in *Strobilomyces floccopus*; spore print deep brown with an olivaceous tinge to blackish, not reddish when fresh.
Gen. *Boletellus* Murr. em.

F. Pileus pink, purple, or carmine, but frequently at least partly expallent, not viscid; spores ornamented with longitudinal wing-like ridges or striate with narrow ribs or veins from the hilar end to the tip, never smooth, nor otherwise ornamented; length of the majority of the spores when fully mature more than 17 μ. Sect. *Ananae* Sing.

F. Pileus rarely showing the above colors, and then either viscid, or (and) spores smooth, or differently ornamented, or shorter.

G. Spores either smooth and very large (length 20 μ and above), or short (around 12 μ) and reticulate; apex of the stipe with a rather indistinct to very distinct reticulation, or most of the stipe with longitudinal veins that anastomose, but never entirely smooth, nor with waxy-costate-lacunose surface in the species known.

H. Spores strongly elongate, smooth; margin decidedly projecting.
Sect. *Mirabiles* Sing.

H. Spores short, reticulate; margin not distinctly projecting.
Sect. *Retispori* Sing.

G. Spores either smaller than 19.5 μ if smooth, or, if they are not smooth, never reticulate; stipe either waxy-costate-lacunose, or smooth, or woolly-shaggy-subreticulate.

I. Neither pileus nor stipe viscid when wet, and veil, if present, not viscid. Sect. *Chrysenteroidei* Sing.

I. Either pileus or stipe or veil (if present) viscid, or all three viscid.

K. Spores with longitudinal wing-like ridges.

L. Stipe smooth or almost so, sometimes annulate; pileus not velutinous-tomentose. Sect. *Ixocephali* Sing.

L. Stipe waxy-costate-lacunose, the thin lamellose ribs very high, limiting deep honeycombs where rain-water remains stored; pileus tomentose (at least in the American species), and not distinctly (if at all) viscid. Sect. *Dictyopodes* Sing.

K. Spores with short cylindric warts which for the most part are imbedded in the exosporium, and when the upper surface of the spores is focussed upon, it appears punctate.
Sect. *Allospori* Sing.

A. Hymenophore lamellate. Gen. *Phyllobolites* Sing.

Strobilomyces Berk., Decades of Fungi 32–33. Hook. Journ. Bot. **3**: 77. 1851.

Eriocorys Quél., Ench. Fung., p. 163. 1886.

Characters of the genus: see key (p. 106). Type species *Strobilomyces strobilaceus* (Scop. ex Fr.) Berk., a synonym of *S. floccopus* (Vahl in Fl. Dan. ex Fr.) Sacc.

Sect. **Genuini** Sing. sect. nov.

Sporis levibus, perforato-verrucosis, cristulatis, reticulatisve.

Characters of the section: see key (p. 106) and the above Latin diagnosis.

KEY TO THE SPECIES

A. Spores smooth or with short perforating warts in the episporium, making the spores appear punctate when seen from above. *S. velutipes*

A. Spores not smooth (only a small minority occasionally smooth, and many immature spores smooth, but then the ornamented ones not as above), and not punctate when seen from above.

B. Spores with short ridges and occasional isolated warts, incompletely connected with each other to form a fragmentary network; pileus with a covering of rigid, very acute, erect, thin spines or pyramids which often are cristate, or fimbriate, or fasciculate, the single ones frequently applanate in one direction, and never with a base wider than 1 mm. along its smallest diameter, and 2 mm. along its largest diameter in dried condition. 1. *S. confusus*

B. Spores with a complete network; pileus with less rigid and more woolly scales with a larger basal diameter when dried, or with the same kind of warts as described above (the scales rather smaller than those), but at the same time with merely squamulose marginal half.

C. Warts or spines or scales of the pileus larger than 2 x 1 mm. at their base, and not very rigid. 2. *S. floccopus*

C. Warts acute, and their base smaller than 2 x 1 mm., rigid, confined to the disk; the marginal half merely squamulose. *S. nigricans*

DESCRIPTIONS OF THE SPECIES OBSERVED IN FLORIDA

1. **Strobilomyces confusus** Sing. spec. nov.

PLATE I, FIG. 1

Pileo atro, spinulis erectis, rigidis, acutissimis, subinde fimbriatis vel cristatis obtecto, spinulis densioribus in centro, margine velo appendiculato, sed haud distincte nec persistenter lanato, sicco, 40–85 mm. lato; tubulis porisque albido-griseolis vel griseis, siccando nigrescentibus, cinnamomeo-maculatis ubi tacti sunt, at maculis eis gradatim nigrescentibus, 10 mm. longis vel longioribus, plerumque subdepressis ad stipitem; sporis in cumulo nigris, cristulatis et incomplete reticulatis, 10.5–12.5 x 9.7–10.2 μ; basidiis 37–48 x 11–13.5 μ; cystidiis clavatis, dein fusoideo-mucronatis vel vesiculosis apice ampullaceo, fuscescentibus, (22)–34–61 x 7.5–26.5 μ; stipite griseo, ad apicem reticulato, lanato-lacerato infra zonam reticulatam, plerumque attenuato basin versus, ad ipsam basin saepe incrassato, solido, 42–78 x 10–20 mm.; velo pro ratione sparso; carne pilei alba, celerrime rubescente, in stipite alba, autoxydatione et rubente et nigrescente; odore haud notabili. Habitatio: in pratulis umbrosis, in hortis, in pinetis mixtis prope *Quercus minimas*, in dumetis frondosis sub *Quercubus laurifoliis* et *Q. virginianis*. Secundum litus Atlanticum Septentrionale occidentemque versus usque ad Ohio et Tennessee.

Pileus between fuscous black and black, with thin but rigid, not woolly, very acute, erect spines which are denser toward the center, and often cristate, fimbriate, or fasciculate, also not rarely compressed, the basal diameter not over 2 x 1 mm. in dried material, occasionally the spines obliterated or evanescent, and then the pileus appearing areolate-squamose and not much different from similar forms of *S. floccopus*, never viscid, not woolly, or at least not strongly and persistently so because of the greater coherence and scarcity of the veil which often hangs in small patches from the margin, surface convex, the center flattened when old, 30–85 mm. broad. — *Hymenophore* whitish-gray to gray, becoming blackish even if carefully dried, becoming spotted with "cinnamon" where touched in fresh condition, these cinnamon places then changing to deep chocolate and finally black; tubes 10–18 mm. long, very slightly depressed around the stipe, but also adnate-subdecurrent in smaller, and decidedly depressed in larger individuals; pores medium wide, about as in *S. floccopus*; *spore print* black. — *Stipe* gray, reticulate at the apex above the annulus (or remnants of the veil), shaggy-woolly below it, solid, tapering downward or more rarely equal, the base often somewhat thickened, 42–78 x 10–20 mm.; veil not abundant, and annulus not persistent. — *Context* whitish, quickly reddening in the pileus reaching a deep carrot color except for a narrow zone under the cuticle, the context of the stipe white, reddening and blackening simultaneously on exposure; odor not remarkable.

Spores short-ellipsoid to globose, fuscous, with very fragmentary dense network, or merely echinate, cristulate with occasionally connected spines, the ornamentation 0.3–1.7 μ high but usually partly or almost entirely imbedded in the much paler exosporium, wall rather thick with an incomplete or complete germinative pore, 10.5–12.5 x 9.7–10.2 μ; basidia (3)–4-spored, 37–48 x 11–13.5 μ; *cystidia* clavate, then becoming fusoid-mucronate and brown, also sometimes clavate-mucronate, or vesiculose with ampullaceous apex (neck), (22)–34–61 x 7.5–26.5 μ; *trama* of the dissepiments of the hymenophore with a colored, denser mediostratum and a paler lateral stratum of divergent hyphae, hyphae without clamp-connections; *cuticle* (tips of the warts) of strands of parallel hyphae which are brown, 17.5–63 x 3–20 μ, with transverse outgrowths, horns, buttons etc., the terminal members sometimes clavate.

Chemical reactions: not separately studied (hardly much different from those of *S. floccopus*).

Habitat: On shaded grass lawns in gardens, in mixed pine woods (e.g. woods of *Pinus palustris* and *Quercus minima*), and also in high and mesophytic hammocks under hardwood trees (e.g. *Quercus laurifolia* and *Q. virginiana*). Summer.

Distribution: From Massachusetts west to Ohio and Tennessee south to central Florida. In Florida not rare though not quite so common as *S. floccopus* which seems to go farther north as well as farther south.

Material studied: **Fla.:** Eustis, Lake Co., Oct. 1897, *Thaxter 6177* (immature) (FH); Flatwood east of Gainesville, Alachua Co., July 1943, *Singer F 2782*, **type** (FH); other

collections in and near Gainesville, *Singer F 2766 I, F 2531, F 2647*, **co-types** (FH). **Mass.** Wood's Hole, *Farlow* (FH); Plymouth, Aug. 1889, *Farlow* (FH).—**Pa.:** Martic Forge, Marticville, on humus in chestnut orchard, Aug. 1910, *J. F. Collins* (FH).—**Ohio:** Symmes, Hamilton Co., July 1935, *W. B. Cooke 41452.* (FLAS, FH).—**Tenn.:** Cades Cove, Blount Co., July 1938, *J. P. Porter*, mixed collection (FH).

This species, so strikingly different microscopically, has never been distinguished from *S. floccopus* (*S. strobilaceus*) in this country. Both these species apparently require about the same conditions for their development since many collections in the herbaria are mixed. Once the distinguishing features are understood, they are so pronounced that it is possible to foretell what type of spores a certain dried specimen will have, and vice versa. Allowance, however, should be made for specimens which under certain conditions, do not develop or preserve the characteristic acute spines on the pileus. These forms are, fortunately, rather rare. On the other hand, the ornamentation of the spores is quite constant. Among published photographs Krieger, *Popular Guide* . . . , p. 444, *fig. 125*, phot. Cummins, may well belong here, while Atkinson, *Mushrooms*, *fig. 172–174* certainly is *S. floccopus*.

2. **Strobilomyces floccopus** (Vahl in Fl. Dan. ex Fr.) Karst., Bidr. Finl. Nat. Folk **37:** 16. 1882.

Boletus floccopus Vahl *in* Fl. Dan. *ex* Fr., Syst. Myc. **1:** 292. 1821; Persoon, Myc. Eur. **2:** 144. 1825. *non* Rostkov.
Boletus gossypinus Pers., Myc. Eur. **2:** 144. 1825.
Boletus squarrosus Pers., Myc. Eur. **2:** 145. 1825.
Boletus cinereus Pers., Myc. Eur. **2:** 144. 1825.
Boletus strobilaceus Scop. *ex* Fr., Elench. **1:** 127. 1828.
Boletus stygius Wallr., Comp. Fl. Germ. Sect. II, **4:** 608. 1833.
Boletus strobiloides Krombh., Nat. Abb. **10:** 21. *pl. 74, fig. 12–13.* 1846.
Strobilomyces strobilaceus Berk., Dec. Fung. 32–33, Hook. Journ. Bot. **3:** 77. 1851; Outl. Brit. Fungol., p. 236. 1860.
Boletus lepiota Vent., Micet. Agr. Bresc., p. 37. *pl. 43.* 1863.
Boletus hydriensis (Hacquet ut *Clathrus ex*) W. Voss, Oesterr. Bot. Zeitschr. **32:** 40. 1882.
Eriocorys strobilacea Quél., Enchir., p. 163. 1886.
Eriocorys strobilacea var. floccopus Bat. Bolets, p. 11. 1908.

PLATE I, FIG. 2.

Pileus between "mummy brown" and black, or about "sepia," or more gray all over or particularly near the margin, the rimosely diffracted portions of the pileus between the scales either much paler, almost white, or even darker (black) if the discoloration of the context has reached the surface, paler on the margin because of the abundance there of the woolly gray or white or sordid veil that hangs in soft patches from the margin and initially continues into the corresponding layer of the stipe, the scales often with blackened tips, softer than in *S. confusus*, entirely covering the surface of the pileus, not viscid, large (larger than 2 x 1 mm. at their base in dried specimens), erect and pyramidal, often curved, or imbricate and more or less appressed, hardly denser toward the center of the pileus, surface pulvinate, finally flattened, 44–155 mm. broad.—*Hymenophore* gray, or white, be-

coming gray, the pores concolorous but reddening or blackening on pressure, or reddening and subsequently blackening, angular, usually lamellose around the stipe when mature, medium to wide, tubes 12–14 mm. long or occasionally longer, adnate to somewhat decurrent, or slightly depressed around the stipe; *spore print* black. — *Stipe* gray to concolorous with the pileus, and with gray or white or sordid abundant veil at least in the middle portion of the stipe where it forms a thick woolly covering, sometimes several subparallel belts up to a usually amorphous annulus, the apex subglabrous, striate with longitudinal veins running down from the walls of the tubes, the base tomentose, solid, subcylindric, or tapering upward, or downward, 50–125 x 12–21 mm.; veil sometimes consisting of two distinct layers, the outer one more colored. — *Context* white or whitish in the pileus, either merely blackening, or reddening while blackening, or reddening and then blackening, these discolorations sometimes blending into a dirty pink or fuliginous-lilac shades, the same colors and color changes taking place in the stipe though the discolorations there quite frequently in contrast with the context of the pileus, rather dry, especially in the stipe when compared with some other species of Strobilomycetaceae and especially with Boletaceae, and slightly tougher than these; odor not remarkable; taste mild, nutty.

Spores 9.7–15 x 8.5–12 μ, covered entirely by a constantly complete network that raises 0.3–1.7 μ high, but frequently almost entirely imbedded, the wall rather thick, with a more or less distinct germinative pore, asymmetrical, buff brown with deeper brown ornamentation; *basidia* 30–58 x (7.5)–10–18 μ, 4-spored; *cystidia* numerous, 17–90 x 8–26 μ, clavate, very frequently mucronate-apiculate, hyaline, occasionally containing a brown drop when very young, later brown throughout, few cystidia more lanceolate or fusoid, but never really acute above; *trama* of the dissepiments of the tubes consisting of a denser, brownish mediostratum, and a looser, hyaline divergent lateral stratum, hyphae of the mediostratum cylindric (few vesiculose), parallel-subinterwoven, clampless; *spines of the pileus* made up of strands of parallel hyphae with clampless septa at a distance of (20)–40–88 μ, cylindric or slightly narrowed at the septa, brown, the terminal members often slightly attenuate toward the apex, but always rounded, 7–17.5 μ thick.

Chemical reactions. **KOH** on surface of pileus brown; on context red then usually becoming brown or brownish, the red reaction usually bright and distinct, rarely dull because of the beginning blackening due to autoxydation; **NH_4OH** on context ochraceous, or (in Dade Co. specimens) nil; **$FeSO_4$** bluish gray, bluish-greenish-gray, or glaucous; **HNO_4** on context yellow.

Habitat: In mixed woods, more frequently under frondose trees than under conifers, in Florida frequently on well-shaded lawns, in gardens, also in mesophytic, high, and tropical hammocks, usually under oaks. Summer and fall.

Distribution: With certainty from Spain to the Caucasus, and from Sweden to North Africa in the Old World, and from tropical Florida to

Canada, apparently west to the Pacific Coast, also observed in eastern Asia (e.g. Japan); its occurrence near the Equator and southward is not quite certain. In Florida probably in all counties.

Material studied: **Fla.:** Dade Co., Fairchild Tropical Garden under *Quercus virginiana* Sept. 1942, *Singer F 750* (FH); Brickell's Hammock under *Q. virginiana*, November 1942, *Singer F 931* (FH); Alachua Co., numerous collections in 1943, *Singer F 2766, 2647a, 2194, 2752, 2020a, 2020b, 2606, 2899, 2532, 2736/II* (all FH), some *Murrill* (FLAS). — **Canada:** Petawawa Forest Experiment Sta. *C. G. Ridley 9531* (FH). — **Me.:** Kittery Point, Aug. 1897, *Thaxter* (FH) and Aug. 1895, *Thaxter 1559* (FH). — **Mass.:** Milton, Aug. 1896, *H. Webster* (FH); Plymouth, *Farlow* (FH); *Hornblower*, det. D. H. Linder (FH); Wakefield, *Singer* (FH); Harvard, *Singer* (FH). — **Pa.:** Center Co. State College, Sept. 1933, *W. L. White 1284* (FH). — **Ga.:** vicinity of Tallulah Falls, July–Sept. 1901, *A. B. Seymour*, det. W. G. Farlow (FH). — **Iowa:** Iowa City, *G. W. Newton*, det. A. P. Morgan (FH). — **Mich.:** Whitmore Lake, July 1921, *L. E. Wehmeyer* (FH). — **Tenn.:** Cades Cove, Blount Co., July 1938, mixed with *S. confusus*, *J. B. Porter* (FH). — **Austria:** Rekawinkel, Niederoesterreich, *Hoehnel* (FH); — **Germany:** Chiemsee, southern Bavaria, Aug. 1922, *Singer*; — **France:** several collections of fresh material; — **Spain:** Catalonia, La Sellera, *Codina*, det. Singer, Aug. 1934 (BC); — **Caucasian Mts.:** Swanetia, Khä, under *Corylus ?colurna*, Aug. 1929, *Singer* (W); Filimonov Range, under *Fagus orientalis* and *Abies Nordmaniana* on decayed wood, Aug. 1935, *Vasilieva* (LE, KAZ.); also other Caucasian localities, once on rhizomata of *Struthiopteris germanica*, etc.

There is no doubt that this species can easily be separated from all other species of this group. Unfortunately, the well-introduced and generally accepted name *S. strobilaceus* is illegal since Fries in his *Systema Mycologicum* used a name *ex Flora Danica*, but later has both species side by side, with the unexpectedly jovial comment (translated): "in my sense not enough different, but I figure, I have to concede a bit to others also." Had it been possible to emend this and similar situations by replacing Fries's *Systema* by the *Epicrisis* as the nomenclatorial starting point, while there was still time (see Revue de Mycologie **3:** 39. 1938), the usual name could have been saved. Since, mostly because of the war, no steps have been taken, I do not want to make my own rules in monographic papers, but I think it is the general desire of all taxonomists that now, since it is too late for drastic changes, no belated halfway measures may punish those who have followed the rules strictly. Many important monographic papers have appeared since my suggestion appeared, and it is to be hoped that their nomenclature, not the nomenclature of older and now less important papers be preserved. As for *S. strobilaceus*, however, there may still be a faint chance to preserve this name for a valid species, if future careful studies should prove that *S. floccopus* and *S. strobilaceus* represent two independent species. I have tried to distinguish between a form with reddening flesh and another one with blackening context, and at first it seemed that a slight difference in the KOH reaction runs parallel with this differentiation; it also appeared that there was a correlation between these characters and the more sepia or more gray color of the pileus, and the shape of the scales; but on the other hand, there seemed to be at least one more form with a different combination of characters (the form from Dade Co.). Intermedi-

ate forms, in the long run, made me abandon my initial scheme. As I see now, these intermediate forms may at least partly be explained by the fact that many of them eventually turned out to be *S. confusus*, but even after their elimination, the distinction between the typical *S. floccopus* and *S. strobilaceus* is not quite clear, and it would be unwise to identify these forms as observed by us with European species described as poorly as *Boletus floccopus* in Fries's *Systema*. Unless further accurate data on a large number of specimens in Europe as well as in America prove the opposite to be true, I treat *S. floccopus* as a variable but homogeneous species.

S. floccopus is considered, by mycophagists, as edible though mediocre in quality.

EXTRALIMITAL SPECIES

Strobilomyces velutipes Cooke & Mass., Grevillea **18:** 5. 1889.
PLATE I, FIG. 6.

Described from Australia, it is said to be "resembling *S. strobilaceus* and *S. nigricans*, but entirely different in the spores." I have studied a fragment of the type (NY). This species actually differs from *S. floccopus* and *S. nigricans* in having spores which are either smooth, or differently ornamented, and smaller than in the other species. I found them short-ellipsoid to subglobose, fuscous, thick-walled, more often without any ornamentation than with it, and, in the latter case faintly rough in the outline of the main wall which is often surrounded by an indistinct hyaline perisporium, 8.5–10.5 x 7–7.7 μ; the ornamentation consists of warts which frequently are so dense as to form an interrupted partial reticulation, the reticulation at least in its lower part imbedded in the main wall much as in *Porphyrellus gracilis* but less conspicuously so because of the low ornamentation and the slight difference in color between the warts and the remaining portions of the wall (ornamentation rarely reaching 0.7μ in height); basidia 24–30 x 10–10.5 μ, clavate-subfusiform and short, 2–3–4-spored, often aborted and cystidia-like; cystidia fusoid and usually apiculate at the apex; hyphae without clamp connections. *S. indicus* Lloyd (Mycol. Notes **7:** 1331. 1924) is, according to the data given by its author, the same species.

S. velutipes demonstrates the transition from smooth to ornamented spores. It does not demand too much imagination to draw a connecting line between the smooth short spores of this species and the elongate smooth spores of *Porphyrellus*, both with a varying percentage of perforately warted spores intermixed in a single preparation, with the same chemical characters (if it can be supposed that the reactions of *S. velutipes* are essentially the same as in *S. floccopus*), the same dull colors, the same habit, etc. The elongate spores, naturally, have the wall slightly thinner and lighter colored. For those who are used to phylogenetic thinking, there remains little doubt that the genus *Porphyrellus* has derived from such forms as *S. velutipes*.

Strobilomyces nigricans Berk., Dec. Fung., Lond. Journ. Bot. **4:** 139. *pl. 6.* 1852.

Described from Khassya, British India, it was rediscovered by Patouillard in one of the collections sent to him from Indo-China. In his Herbarium, now preserved at the Farlow Herbarium, the dried specimens described by him (Bull. Soc. Myc. Fr. **39:** 54. 1923) are found in good condition though still labeled with an herbarium name since Patouillard initially thought this species was new. The acute warts of the center of the pileus remind one of *S. confusus,* but *S. nigricans* is smaller, at least the Indo-Chinese collection, and the warts are denser. When compared side by side with either of the American species, it seemed to be different. The spores were found to be identical with these of *S. floccopus:* dark fuscous, thick-walled, short-ellipsoid to subglobose or globose, with a complete network of arcuate ridges and veins which are up to 0.5 μ high, 9–11.5 x 8.5–10.3 μ.

The existence of this species shows that *S. floccopus,* usually characterized as a very isolated form, is only one out of a whole series of closely related species.

SPECIES IMPERFECTLY KNOWN

Strobilomyces polypyramis Hook. fil. *apud* Berk., Dec. Fung., Hook. Journ. Bot. **3:** 78. 1851.

The type comes from India. I have not seen any specimens. This is probably another species of this section.

Strobilomyces montosus Berk., Dec. Fung., Hook. Journ. Bot. **3:** 78. 1851.

The type comes from India. I have not seen any specimens. This may be another species of the *Floccopus*-complex.

Strobilomyces echinatus Beeli, Bull. Soc. Roy. Bot. Belg. **58:** 212. *pl. 15, fig. 11.* 1926.

The type comes from the Belgian Congo. I have not seen any specimens, but this species appears to be remarkable because of the acuminate disc, and the punctate spores (according to Gilbert, Bull. Soc. Mycol. Fr. **52:** 258. 1936). It probably belongs in this section.

Sect. **Pterospori** Sing. sect. nov.
Sporis de apice usque ad hilum alato-costatis.

Characters of the section: see key (p. 106) and the above Latin diagnosis.

Strobilomyces pterosporus Sing. spec. nov.

PLATE I, FIG. 8.

Pileo umbrino-fuligineo, inter squamas subpallidiore, squamis umbrino-atris, appressis maculato, plus minusve 40 mm. lato, convexo; tubulis griseo-brunneolis in adultis, longissimis (radio pilei longioribus), circum stipitem depressis, poris concoloribus, irregularibus, compositis, angulatis, amplis, superficiem hymenophoralem ventricosam efformantibus; sporis fuscis vel melleo-fuscis sub microscopio, atris in cumulo, longitudinaliter (6)–12-costatis vel -alatis, ornamentatione 1.6–2 μ alta, ad polum apicalem abrupte levibus, 16.5–21 x 14–14.5 μ; basidiis tetrasporis, circa 25 x 17 μ; cystidiis

versiformibus; hyphis tramalibus haud fibuligeris; stipite concolori, centrali, plus duplico longiore quam diameter pilei, e velo ipsum apicem stipitis approximante fibrosotomentoso, cylindrico, solido; carne pallida, mutabili, tenui (1–3 mm.) in pileo. Habitatio: Ad terram in silvulis intersecatis apricis Augusto mense. In Liberia Africae Tropicalis Occidentalis, Firestone # 3, Du River. *D. H. Linder 312* (FH).

Strobilomyces costatisporus (Beeli) Gilb., Bull. Soc. Mycol. Fr. **52:** 260. 1936.
Boletus costatisporus Beeli, Bull. Soc. Roy. Bot. Belg. **59:** 162. 1927.

Described from the Belgian Congo, this species differs from *S. pterosporus* in being larger, but having shorter tubes and smaller pores, and also definitely smaller spores. I have not seen any specimens. *S. lepidellus* Gilb. (Bull. Soc. Myc. Fr. **52:** 259. 1936) nom. subnud., has, as far as I know, not been validly described since, and therefore cannot be compared with either *S. pterosporus* or *S. costatisporus*.

Porphyrellus Gilb., Bolets, p. 99. 1931.

Phaeoporus Bat., Bolets, p. 11. 1908, *non* Schroeter, Krypt. Fl. Schles. (1888).

Boletus subgen. *Porphyrosporus* Smotlacha, Monog. Cesk. h. Hřibov. p. 31, 1911.[2]

Characters of the genus: see key (p. 106). Type species *Boletus porphyrosporus* Fr., i.e. *Porphyrellus pseudoscaber* (Secr.) Sing.

Sect. **Tristes** Sing. sect. nov.
Pileo haud laete colorato; sporis plerumque levibus; carne fortiter colorata reagentium alcalinorum ope.

Characters of the section: see key (p. 107), and the above Latin diagnosis. Type species *Boletus tristis* Pat. & Baker.

DESCRIPTION OF THE SPECIES OBSERVED IN FLORIDA

Porphyrellus pseudoscaber (Secr.) Sing. comb. nov. ssp. **typicus.**

Boletus pseudoscaber Secr., Mycogr. Suisse **3:** 13. 1833.
Boletus porphyrosporus Fr. in Hök, Boleti, p. 13. 1835.
Krombholzia porphyrospora Karst., Hattsw., p. 17. 1882.
Gyroporus porphyrosporus Quél., Enchir., p. 162. 1886.
Boletus nebulosus Peck, 51st Rep. N. Y. St. Mus., p. 292. 1898.
Boletus umbrosus Atk., Journ. Myc. **8:** 112. 1902. *sec.* Snell.
Phaeoporus porphyrosporus Bat., Bolets, p. 11. 1908.
Tubiporus porphyrosporus Ricken, Vademecum, p. 205. 1918.
Porphyrellus porphyrosporus Gilb., Bolets, p. 99. 1931.

There is no need for a detailed description of the type subspecies, since Kallenbach has given a fairly complete account on the European species, and Snell has described the American collections (see the following illustrations: Die Pilze Mitteleuropas, pl. 26, *fig.* 1–13, and Mycologia **24:** 336. 1932). I found the spores (average of several measurements on various materials from Austria) 13.5–19–(20.4) x 6.3–8.5–(10.2) μ, and among

[2] *Porphyrosporus* Smotlacha has been incorrectly cited as a genus by both Gilbert and Kallenbach.

them some smaller spores, several of which (among several hundreds of normal spores) have warts imbedded in the pallid exosporium, 8–9 x 5.4–6 μ; basidia 34–41 x 13.5–15.3 μ, 4-spored; cystidia fusoid-mucronate with the tip rather acute or quite acute, hyaline or more often brown, especially in old specimens; trama of the hymenophore and structure of the cuticle exactly as in ssp. *cyaneocinctus* Sing. (p. 117), the hyphae of the trichodermium are illustrated in Kallenbach, l.c., *pl. 33, fig. 47* [10,11,15]. Old herbarium material from Europe (*Hoehnel*, FH) did, however, not show a true trichodermium. This latter was preserved in but a few fragments, in others the repent thin cylindric hyphae of the surface (up to 6.5 μ in diameter) were either the remaining base of the trichodermium after it had been rubbed or washed off, or such a trichodermium is not constant in European plants. The context of the European form is also variable. Sometimes there is no bluing at all, and these forms are mostly found in coniferous woods (see Singer, Pilze aus dem Kaukasus, Beih. Botan. Centralbl., Abt. II, **48:** 519. 1931); they also were described as a variety, *Boletus porphyrosporus* var. *minor* Bat. & Crawshay (Bull. Soc. Myc. Fr. **39:** 267. 1924) but the decision whether these variations are indications of the presence of a collective species in Europe, or merely fluctuations within an homogeneous species, must be left for European mycologists. W. H. Snell's American collections have, notwithstanding statements to the contrary by the original authors of evidently identical species, all more or less bluing flesh, and seem to represent a single form, certainly identical with at least a part of the European *P. pseudoscaber* ssp. *typicus*. In the type of *Boletus nebulosus*, the larger spores are thin-walled, and the smaller ones thick-walled, fusoid or ellipsoid, or constricted; the dimensions of the spores are extremely variable as far as the length is concerned (variation of 100%), but little variable in breadth: 11–20.5 x 5.8–7.5 μ, mostly 12.5–15.5 x 6–7 μ; basidia large, 4-spored; cystidia brown often clavate, often gigantic; trama truly bilateral as in ssp. *cyaneocinctus*; epicutis of erect to repent clampless hyphae, some fascicles of hyphae forming fragmentary trichodermia, terminal members of the chains 5–11 μ in diameter, subfusoid, or clavate, not subulate. When trying to distinguish the typical form of Europe and of the northern part of the Atlantic Coast States from ssp. *cyaneocinctus*, one should keep in mind that most boletes tend to become rimose-tessellate in age, and unless several observations on many specimens corroborate the first observations, the positive finding should not necessarily mean that the form studied is ssp. *cyaneocinctus*. It is also important to note that the characteristic color ring of the latter subspecies does not serve to distinguish dried specimens, since it disappears on drying.

3. **Porphyrellus pseudoscaber** (Secr.) Sing. ssp. **cyaneocinctus** Sing. nom. nov.
Boletus fumosipes Peck, Ann. Rep. N. Y. St. Mus. **50:** 108. 1897, *pro parte*.
Ceriomyces fumosipes Murr., Mycologia **1:** 154. 1909.

Pileus "snuff brown" to "Saccardo's umber" with paler portions, velutinous or subvelutinous, cracking readily into areoles so that all adult speci-

mens are conspicuously rimose-tessellate, the crevasses whitish and running in all directions, not viscid, convex, 25–115 mm. broad. — *Hymenophore* between "cartridge buff" and "cream buff," becoming grayer, and eventually with a tinge from the spore color, greenish blue on injury, tubes up to 21 mm. long, deeply depressed around the stipe, leaving no sterile margin outward, pores forming a very convex outer-lower surface, concolorous with the tubes, and discoloring on injury the same way as these, the discolored patches finally becoming chestnut brown, diameter up to 2 mm. i.e. rather wide when mature; *spore print* "warm sepia" in a good print. — *Stipe* pale yellowish at the very apex with lamelloid proliferations of the walls of the tubes which run down to a characteristic though sometimes indistinct greenish blue to green ("deep lichen green" to "Rejane green") color ring below which the stipe is "wood brown," rarely without an apical coloring, extremely faintly fibrillose or velutinous on pale yellowish ground, later becoming "Saccardo's umber," subequal usually with thickened base, solid, 46–70 x 12–16 mm. — *Context* white, on exposure staining "porcelain blue" to "gobelin blue" near the tubes, but almost entirely bluing when young and fresh specimens are squeezed, the expressed sap coloring white paper blue, slightly dirty lilaceous under the cuticle, mostly more or less bluish in the apex of the stipe; odor none; taste somewhat sour.

Spores variable in shape, usually more or less fusoid, also ellipsoid-oblong, 10.5–14.3–(18) x (4.5)–6–(7.7) μ, with suprahilar depression, rarely without it, usually with a wall consisting of a brownish inner layer and a hyaline or pallid, thin exosporium which looks like an optical halo when well developed and seen under oil immersion, the exosporium usually optically empty, and then the spores definitely smooth, or in some rare cases perforated with thin cylindric spines which do not emerge above the surface of the exosporium but make the spore appear punctate when the upper surface is in focus, the walls occasionally seemingly thick but this is caused by the invisibility, in some instances, of the inner surface of the main wall so that the latter seems to extend from the surface of the spore to the surface of the oil drop, but occasionally actually thick, especially in the apical, rarely in the basal portion; *basidia* 4-spored, 30–36 x 9.5–14.5 μ; *cystidia* fusoid, or broadly ventricose-mucronate, but obtusate, hyaline to brownish, 35–58 x 10–20 μ; *trama* of the young tube walls truly bilateral, but very soon becoming xerocomoid, i.e. bilateral-subregular, the mediostratum initially darker and denser, soon all hyphae equally colored and equally dense, lateral stratum initially divergent, soon divergent but in a narrow zone where the trama turns into the subhymenium; *cuticle* a trichodermium, consisting of a hymeniform epicutis with dermatocystidioid terminal members of erect hyphae, arising from a subcuticular layer of more or less erect hyphae which frequently are to a certain extent broken down into chains of almost cubical to globose elements, and have a larger diameter than the basal septum of the cystidioid elements of the epicutis, the latter (dermatocystidia) subulate, a smaller number of them fusoid, the very apex rounded, 26–42 x 7–14 μ, hyaline, smooth.

Chemical reactions. **KOH** on surface of pileus purplish to reddish but not as bright as with NH_4OH; on context "Etruscan red," becoming "vinaceous tawny." — **NH_4OH** on the surface of the pileus deep purple ("dark livid brown"), the center of the reacting spot eventually "English red"; on context at some places reddening then browning, at others sordid (little reaction). — **HNO_3** on context brownish. — **$FeSO_4$** "deep glaucous gray" or "dark glaucous gray." — **Formol** salmon color. — **Methylparamidophenol** negative.

Habitat: Under oaks on the soil, gregarious. May to August. Rather frequent though not abundant anywhere.

Distribution: New York to North Florida, and "west to Kentucky" according to Murrill. In Florida known only from Gainesville, but certainly of quite general occurrence in North Florida.

Material studied: **Fla.,** Alachua Co. Gainesville, under Evergreen oak (*Quercus laurifolia*), Aug. 1937, *W. A. Murrill* (*Boletus fumosipes*) (FLAS, **co-type**); also other collections in high hammocks under oaks, June 1–July 25, 1943, *Singer F 2193, 2607, 2825* (FH, **co-types**). — **N. Y.: Type,** also **type** of *Boletus fumosipes*, p.p. (NYS).

As pointed out by Snell, a portion of the type of *B. fumosipes* Peck has striate spores and belongs to *Boletellus chrysenteroides* Snell, while another portion has smooth spores and this seems to be the basis of Peck's description. However, the fact remains that the type of *B. fumosipes* is a mixed sample, and no type specimen is designated. The author therefore preferred to avoid Peck's name by choosing a new name for the subspecies described above which has been called *Boletus fumosipes* by Murrill, and *B. sordidus* by Coker and Beers.

EXTRALIMITAL SPECIES

Porphyrellus tristis (Pat. & Baker) Sing. comb. nov.

Boletus tristis Pat. & Baker, Journ. Straits Branch R. Asiatic Soc. No. 78: 70. 1918.

I have studied the type in the Patouillard Herbarium (FH). This is an interesting species. The stipe is subvelutinous, somewhat thickened at the base, the tubes are subadnate at maturity, wide in age, 3.5 mm. long when fresh. The spores are fusoid to boat-shaped, porphyry-umbrinous, darker than all other elongate spores known in this family and in the Boletaceae, reminding one of *Psathyrella* (*Psathyra*), transparent, smooth, some attenuate almost from the base up, wall thin though not extremely so, the outer layer in some cases slightly obscurely cloudy-punctulate with paler and darker brown zones, 14–17–(19) x (3)–4–5.5 μ, with or without a suprahilar depression; basidia 4-spored, 9.5–12.5 μ broad. The cystidia are fusoid with rounded tips, or subulate, variable in size, hyaline, crowded at the pores. The trama of the tube walls is truly bilateral with a melleous to mahogany brown rather narrow mediostratum, and a hyaline, strongly divergent, somewhat looser, lateral stratum. The cuticle of the pileus is characterized by erect dermatocystidioid terminal hyphae which form an

epicuticular trichodermium-palisade; they are brown, fusoid-ventricose and rounded above, or fusoid-mucronate, 70–100 x 14–28 μ. The velvet of the stipe is formed by thin brown erect hyphae with a capitate-balloon-shaped or clavate terminal hypha, 15–28 x 9–10 μ (very characteristic). All hyphae have clampless septa.

The flesh of this species is said to be white and unchanging, but the same statement has been made on other species of this group. The color and the whole appearance strongly suggest *P. pseudoscaber* which is the nearest species we know. The strange heterogeneity of the exosporium of many spores is indicative for the origin of the ornamentation of the spores in the Strobilomycetaceae by alternating condensations in the exosporium.

This species has been abundantly collected in Singapore but not elsewhere.

SPECIES IMPERFECTLY KNOWN

Boletus sordidus Frost, Bull. Buff. Soc. Nat. Sci. **2:** 105. 1874.
Ceriomyces sordidus Murr. N. Am. Fl. **9** (3): 149. 1910.

Boletus sordidus reminds one of both the type subspecies and the more southern subspecies *cyaneocinctus* Sing. of *Porphyrellus pseudoscaber*. The spores are 10–15–(17.5) x (4)–5.5–7.5–(9.7) μ much the same as in *P. pseudoscaber* if the measurements are expressed in range limits as above, but actually differing in being enormously variable as well in length as in breadth, some spores being as short as 10–11.5 x 6–7 μ, approaching the shape of the most elongate spores of *Strobilomyces velutipes*. The walls of the broadest spores of *B. sordidus* are very thick and have more or less incomplete germ pores at the apex which may also be applanate as in *Ganoderma*, the walls reaching 1.7 μ in diameter at the apical end.

The above data have been taken from authentic material received at Farlow Herbarium from the Boston Society of Natural History where some of the types of Frost's were preserved; it is very probable that this specimen actually represents a portion of the type collection. The type has been described from Vermont, but fresh material has thus far been re-collected neither there nor elsewhere in New England, and it is to be feared that *B. sordidus* is merely a form with abnormal spores of one of the two established subspecies of *Porphyrellus pseudoscaber*, either as Snell first thought (l.c.) ssp. *typicus*, or as he later says (Mycologia **28:** 475. 1936) *Boletus fumosipes* Peck, i.e. *Porphyrellus pseudoscaber* ssp. *cyaneocinctus* Sing. As it seems to me, *Boletus sordidus* has to remain among the dubious species of *Porphyrellus*, sect. *Tristes* until further evidence shows its correct position.

Sect. **Graciles** Sing. sect. nov.

Pileo laete colorato; sporis perforato-punctatis; stipite subreticulato vel lacunoso-favoso; actione acidorum et alkalium haud intensa.

Characters of the section: see key (p. 107) and the above Latin diagnosis.

KEY TO THE SPECIES

A. Stipe merely finely and sometimes obscurely, reticulate, not lacunose. *P. gracilis.*

A. Stipe deeply anastomosing costate, and lacunose because of these anastomoses, the ribs lamellose in shape and waxy in consistency.
B. Spores 14.5–18 x 6.5–8.2 μ. Pileus 47–107 mm. in diameter.
4. *P. subflavidus*

B. Spores 10–14 x 5–6.8 μ. Pileus 40 mm. broad. *P. malaccensis*

4. **Porphyrellus subflavidus** (Murr.) Sing. comb. nov.
Tylopilus subflavidus Murr., Mycologia **30** (5): 520. 1938.
Boletellus subflavidus Snell, Mycologia **33** (4): 422. 1941.
PLATE I, FIG. 4.

Pileus yellowish to almost argillaceous, or whitish, or with pallid center, most frequently about "Massicot yellow" and eventually often "clay color" when the weather is dry, occasionally with a slight "pale ochraceous buff" or "pale ochraceous salmon" tinge, flocculose-velutinous, usually becoming rimose-areolate, eventually frequently fibrose-squamose, not viscid, convex, at length slightly depressed, 47–107 mm. broad. — *Hymenophore* pallid grayish with a vinaceous tint, eventually almost as in *Tylopilus conicus*, very convex or forming a wall near the depression which surrounds the stipe, tubes long (11–21 mm.), pores concolorous, rather wide; spore print on white paper "warm sepia," in thinner layer "Army brown" or "Verona brown." — *Stipe* yellowish white, pale yellow, or light yellow, lamellose to alveolate-lacunose, solid, occasionally becoming hollow in age, cylindric, or tapering down, the base usually acuminate, more rarely bulbose, 45–145 x 7–30 mm. — *Context* pure white, in the base of the stipe usually rich yellow, unchanging in all parts; odor slight, fruity, or almost none; taste bitterish.

Spores melleous with brown markings, somewhat less rich-colored than in *Boletellus betula*, with subsmooth to crenulate outline, the exosporium with imbedded, thin, cylindric spines which when seen from above make the spore appear punctate, fusoid, with a slight suprahilar depression, 14.5–18–(20.5) x (6)–6.5–8.3–(8.8) μ; *basidia* 4-spored, 33–38 x 11.5–15.5 μ; *cystidia* not as striking as in most species of this family, versiform, i.e. clavate-fusoid, irregularly fusoid, elongate-claviform, fusoid-ampullaceous, etc., moderately numerous, 27–54 x 5.5–10.2 μ, hyaline, not incrusted; *trama* of the tube walls truly bilateral, the mediostratum thin, very pale melleous, denser than the strongly diverging, loose, pure hyaline lateral stratum, hyphae without clamp connections, somewhat wavy in the mediostratum, and somewhat gelatinizing in the lateral stratum, multi-septate, especially in the mediostratum and, as usual, in the subhymenium; *cuticle* of the pileus consisting of a trichodermium, the erect hyphae of the outermost layer with ascendant or irregular basal hyphae, more or less fasciculate, loose, and not parallel, with cylindric, cylindric-subcapitate, or clavate terminal hyphae which are neither colored nor incrusted, and have a not extremely thin wall, broadly rounded above, 30–78 x 3.5–11.5 μ; lamellae of the stipe formed by rather parallel, thin, tender, thin-walled, cylindric,

multi-septate, clampless hyphae of 3–3.5 μ diameter, the surface formed by somewhat inflated, hyaline dermatocystidia e.g. 38 x 11.3–11.5 μ.

Chemical reactions: **KOH** on pileus cinnamon buff; on context negative; **NH_3** on pileus avellaneous; **NH_4OH** on pileus cinnamon buff; on context negative; **HNO_3** on pileus yellow; on context negative; **$FeSO_4$** on context negative.

Habitat: In high hammock vegetation, under *Quercus virginiana* and *Q. laurifolia*, and in flatwoods under *Pinus palustris* with *Quercus minima*, on the sandy soil, usually gregarious. June to October.

Distribution: Northern Florida.

Material studied: Alachua Co., *Murrill* (FLAS, **type**); authentic material (FH); *Singer F 2200, F 2539, F 2673, F 2842* (FH).

A well-defined species among the Strobilomycetaceae of this country, but closely allied to *P. malaccensis* and *P. lacunosus*.

EXTRALIMITAL SPECIES

Porphyrellus gracilis (Peck) Sing. comb. nov.

Boletus gracilis Peck, Ann. Rep. N. Y. St. Mus. **24:** 78. 1872.
Tylopilus gracilis Henn. in Engl. & Pr., Nat. Pfl. **1** 1**: 190. 1897.

PLATE I, FIG. 5.

I have studied authentic material sent to Patouillard by Peck (from Ballston, N. Y.). This material seems to be identical with the Albany types (NYS). It is remarkable that no author has as yet seen the characteristic structure of the spore wall which is perforated, as in *Ganoderma*, by brown cylinders, scarcely projecting above the surface of the exosporium, but making the spore clearly appear punctate when the upper surface is focussed upon; they are 11–15.3–(17.5) μ long and (5.5)–6.5–6.7 μ broad, melleous, basidia four-spored, 24–30 x 9.8–11.8 μ. The cystidia are versiform, similar to these of the preceding species. Macroscopically, this species resembles a new species of *Tylopilus*, related to *T. felleus* which will be described in the second part of this paper. Old spore preparations are of the same color as in *P. pseudoscaber*, not as in *Tylopilus*. Coker and Beers indicate the spores as being "hazel" but a four-year-old good spore print from a specimen of Coker's was found to be "Sudan brown" by the writer. *P. gracilis* occurs in New York, New England, and North Carolina, and is said to be distributed "from Nova Scotia to Georgia." It has, as *Tylopilus gracilis* (Peck) Henn., been indicated by Murrill (Florida Boletes, Mim. Contrib. Herb. Univ. Fla. Agr. Exp. Sta., p. 2, 1942) as occurring in northern Florida. However, I was unable to find any evidence in the Herbaria consulted.

Porphyrellus malaccensis (Pat. & Baker) Sing. comb. nov.

Phylloporus malaccensis Pat. & Baker, Journ. Straits Branch R. Asiatic Soc. No. 78: 71. 1918.

It is hard to understand why this species has been published as a *Phylloporus*. It has nothing in common with that genus, and belongs in the

closest neighborhood of *P. subflavidus*. The type from Singapore, which is deposited in the Patouillard Herbarium (FH), is somewhat smaller than the Florida species, and has smaller spores, 10–14–(17.5) x 5–6.8 μ. No other difference between these two species can be discovered. Nevertheless we consider them as independent species.

SPECIES IMPERFECTLY KNOWN

Porphyrellus Cookei (Sacc. and Syd.) Sing. comb. nov.
Boletus lacunosus Cooke & Mass., Grevillea **18:** 5. 1889.

The type, which I have not seen, comes from Australia. This species evidently belongs in the *P. malaccensis-subflavidus*-group. If the spores are described correctly, this species would be distinguished easily by the spore measurements which are given as "15 x 10 μ" (Cooke & Massee). If it were not for this one character, I would be unable to point out how it could be distinguished from *P. subflavidus* unless "pileo subviscoso" indicates an actual difference in the structure and consistency of the cuticle.

SPECIES EXCLUDENDAE

Porphyrellus alboater (Schw.) Gilbert, Bolets, p. 99. 1931. This is a *Tylopilus* (Boletaceae).

Porphyrellus nigrellus (Peck) Gilbert, Bolets, p. 99. 1931. This is the same as *Tylopilus alboater* (Schw.) Murr.

Porphyrellus indecisus (Peck) Gilbert, Bolets, p. 99. 1931. This is a *Tylopilus* (Boletaceae).

Boletellus Murr., Mycologia **1:** 10. 1909, em. Gilbert, Bolets, p. 106. 1931.
Boletogaster Lohwag, Beih. Botan. Centralbl. **42** (*2*): 274. 1931.
Frostiella Murr., Mim. Contrib. Herb. Univ. Fla. Agr. Exp. Sta. Jan. 5, p. 6. 1942; Snell, Mycologia **34:** 409. 1942 (nomen nudum).

Characters of the genus: see key (p. 107). Type species *Boletellus ananas* (Curt.) Murr. (*Boletus ananas* Curt.).

Section **Ananae** Sing. sect. nov.
Pileo roseolo, purpurascente, rubello, rubido-vaccineo, saepe fortiter squamoso vel squamuloso-furfuraceo, haud viscido, margine velo appendiculato vel subnudo; sporis elongatis, longitudinaliter striatis vel vittatis, magnis (i.e. magnitudine majoritatis sporarum maturarum 17 μ excedente).

KEY TO THE SPECIES

A. The longitudinal ridges of the spores high (projecting about 1 μ above the surface of the spore); stipe sometimes red, but not red-tomentose above, nor squamulose, nor furfuraceous; pileus imbricate-squarrose, bearing large scales (see B1, B2, B3).

B1. Stipe umber brown; carpophore small. *B. porphyrius*

B2. Stipe pallid or white, sometimes becoming red at the apex or gray at the base, but never prevalently umber brown; carpophore medium to large. 5. *B. ananas*

B3. Stipe yellow above, red below; carpophore medium to large. *B. emodensis*

A. The longitudinal ridges of the spores usually low (projecting much less than 1 μ, or occasionally just reaching 1 μ height), never transversely veined-striate; stipe either red furfuraceous to tomentose above, or squamulose or furfuraceous; pileus minutely squamulose to glabrous, or tomentose-scrobiculate.

C. Stipe longitudinally striped red, white tomentose at the base, red tomentose-furfuraceous above, 100 x 12–13 mm.; pileus uneven, scrobiculate and tomentose, not scaly, not glabrous, 60–70 mm. broad. *B. obscurecoccineus*

C. Stipe not striped with red, not red tomentose-furfuraceous above, but distinctly furfuraceous or transversely squamulose, 30–50 x 4–9 mm.; pileus minutely squamulose or glabrous, 20–40 mm. broad. *B. cubensis*
B. lignatilis
B. guadelupensis

DESCRIPTION OF THE SPECIES OBSERVED IN FLORIDA

5. **Boletellus ananas** (Curt.) Murr., Mycologia **1:** 10. 1909.
Boletus ananas Curt., Amer. Journ. Sci. II **6:** 251. 1848; Berk., Hook. Journ. Botan. **1:** 101. 1849.
Strobilomyces pallescens Cooke & Mass., Grevillea **18:** 5. 1889.
Boletus isabellinus Peck, Bull. Torr. Club **24:** 146. 1897.
Boletellus pallescens Gilbert, Bolets, p. 107. 1931.
?*Boletus coccineus* Fr., Epicr., p. 423. 1838.
?*Strobilomyces coccineus* Sacc., Syll. **6:** 50. 1888.
?*Boletus ananaeceps* Berk., Linn. Soc. Lond. Journ. (Bot.) **13:** 161. 1873.
?*Strobilomyces ananaeceps* Sacc., Syll. **6:** 50. 1888.
?*Secotium excavatum* Kalchbr., Ertek Termész. Köreb Kiadja a Magy. Tud. Ak. **13** (8): 7. *pl. 3.* 1884.
?*Strobilomyces excavatus* Henn., Hedwigia **43:** 187. 1904.
PLATE I, FIG. 7.

Pileus with "deep vinaceous," or "Pompeian red" and "Isabella color" fibers, a general color of about "russet vinaceous" resulting, bleaching when old, fascicles of the fibres forming large woolly-squarrose or subimbricate scales which are the more erect the nearer they are to the center, non-viscid, subglobose at first, becoming convex-expanded, 45–95 mm. in diameter; margin extended into a thick membranaceous veil which is white or nearly so, and initially runs down the apex of the stipe in two indistinct layers, the outer one thin and fragmentary, later breaking off the stipe to leave a sleeve just above the surface of the stipe, finally becoming ragged to form a sordid and pale stramineous, irregularly crenulate-denticulate appendiculation on the margin; these remainders of the appendiculate veil disappear at or after maturity. — *Hymenophore* yellow, often tinged with reddish brown, staining blue where injured, eventually becoming yellowish brown or reddish brown all over; tubes depressed around the stipe, 11–16 mm. long; pores concolorous, large, irregular, angular, forming a distinctly convex lower surface; spore print black or nearly so. — *Stipe* white, or pallid, often with a red belt on the apex ("Pompeian red" or lighter, corresponding to the blue or green belt of *Porphyrellus pseudoscaber* ssp. *cyaneocinctus*), described as grayish below (Cleland for Australian specimens), but only becoming pinkish sordid or sordid when old in American specimens, subfibrillose-subglabrous to fibrous, but rarely with an actual annular remnant

of the veil on the stipe, not scaly-floccose nor shaggy, smooth, tapering upwards when young, later rarely with subbulbous base, or sometimes tapering downwards, or more frequently subequal, solid, 60–140 x 8–15 mm. — *Context* yellowish white or whitish yellow in the pileus, white in the stipe or only in the base of the stipe, readily and strongly bluing when cut, at least in the pileus, occasionally reddening in the stipe; odor none; taste mild.

Spores 13.8–25.5 x 7–11 μ, mostly 17.5–22.5 x 7.5–9.2 μ in American specimens, with strongly projecting longitudinal, slightly spiralling, lamellosely-thin wing-like ridges which abruptly are cut off before they reach the indistinct germinative pore, leaving a very narrow hole at the very apex of the spore, the sides of the ridges and the bottom of the furrows between them transversely venose-striate, the ridges numbering from 12 to 14, and measuring from 0.6–1.2 μ, mostly 1 μ in height; *basidia* 22.5–50 x 9.5–17.5 μ, 2- to 4-spored; *cystidia* fusoid, mucronate, or ampullaceous to ampullaceous-capitate, hyaline, brittle, 24–50 x 5–15 μ; *trama* of the tube-walls truly bilateral with more colored mediostratum and looser, divergent lateral stratum, the hyphae without clamp connections; hyphae of the outer layer of the *veil* hyaline, semiopaque, long-cylindric, irregularly interwoven, thin-walled, 4–7 μ thick, the septa without clamp connections, the terminal members of the hyphae long, with attenuate though rounded, or suddenly broadly rounded, or subcapitate tips.

Chemical reactions: **KOH** on context yellow, becoming brown; on the surface of the pileus olive-yellow, becoming maroon; **NH_4OH** on the surface of the pileus olive-yellow; **$FeSO_4$** on the context green; **aniline** negative; **chlorovanilline** on context becoming yellow where the context had been white (probably only the acid — HCl — reacting).

Habitat: In various kinds of woods, from open pine-oak woods to dense tropical hammocks, frequently on the bases of trees (*Pinus palustris*, *P. caribaea*, *Quercus virginiana*, *Q. Catesbaea*, *Nectandra coriacea*), or on shaded stone walls, climbing at times as high as three feet above the ground, but also on sandy soil under oaks or pines, solitary to gregarious. From May to November in the Northern Hemisphere, December to May in South Australia (according to Cleland).

Distribution: Southern States of the U. S. A., from South Carolina to the south tip of Florida and west to Alabama and Mississippi, probably reaching Mexico and the South Pacific Islands but known with certainty only from New Caledonia, also in Queensland and South Australia. In Florida through all zones.

Material studied: **Fla.:** Dade Co. Matheson Hammock and Fairchild Tropical Garden, September to November 1942, *Singer F 709*, *F 709a*, *F 709b*, *F 1391* (FH); Lake Co., Eustis, Oct. 1897, *Thaxter 2444*; Alachua Co., Gainesville, August 1937, *Murrill* (FLAS, FH); Sugarfoot Hammock, July 1943, *Singer F 2541*, *F 2541a* (FH); Newnan's Lake, July 1943, *Murrill* (FLAS); Madison Co., Cherry Lake, July, *Murrill* (FLAS); Marion Co., Goldhead Park, May, *Murrill* (FLAS). — **S. C.:** Santee River, **type** of *Boletus ananas* Curt. (Curtis Herbarium, FH). — **Ga.:** North-east Georgia, vicinity of the Tallulah Falls (Tallulah Falls, Bad Branch, Sunset Rock, Rock Moun-

tain), from August to September, *A. B. Seymour & W. L. Moss*, det. W. G. Farlow (FH). — **Ala.**: *Earle* (FH). — **Miss.**: **Type** of *Boletus isabellinus* (NYS). — **New Caledonia** (Nouvelle Calédonie): on iron-rich soil, *Le Rat, 1808*, det. Patouillard (*S. pallescens*), (FH). — **Queensland,** Australia: **Type** of *Strobilomyces pallescens* (NY).

Coker and Beers, and also Murrill, Earle, and others emphasized the growth of the carpophores on the base of trees, exclusively pines as it was understood by these authors. Earle supposed parasitism, and Murrill based the genus *Boletellus*, among other characters, on the epixylous growth of the carpophores. This, however, is not in full agreement with the facts. I have in numerous careful observations never been able to find any injury on trees that could be attributed to this fungus, nor is it true that this species is always epixylous, or confined to pines. At some stations, there were absolutely no pines in the vicinity, and at others, the carpophores definitely preferred the bases of oak trees to those of pines in a mixed stand. The fact that many specimens grew, of all places, on scanty humus and débris on stone walls, the mycelium appressed to the rocks, seems to show that *Boletellus ananas* merely prefers to grow along hard surfaces, but most certainly it is not a parasite unless, of course, the mycelium is connected with the roots, and has loose mycorrhizal relationship with oaks and pines, with possible disturbances of the mutual character of the symbiotic relations in favor of the fungus, as it happens under certain conditions with other boletes. However, the lack of specialization on the part of *B. ananas*, as well as the growth on stone walls instead of close to the roots of the trees, or on the ground under trees, makes it highly dubious whether it is a mycorrhizal fungus at all. Without experimental studies proving the fact of actual formation of mycorrhiza, field observations are not sufficient to answer this particular question. At any event, the idea that *B. ananas* is an exclusive parasite of pine trunks must now be abandoned. Another interesting fact about *B. ananas* is the geographic area occupied by this species. If it is interrupted, this would by itself constitute a rare and interesting phenomenon, but it may well be that the many little explored regions and small islands between the known areas, after more exploration, will furnish a bridge between the American and the Australo-Oceanic centers of distribution.

The spores seem to average a trifle larger in the Australian plant, but we obtained two practically identical measurements from the New Caledonian material in Patouillard's Herbarium and from our Dade Co. (Fla., U. S. A.) collections: 19–22.5 x 8–9.5–(11) μ in New Caledonia, and 18–22.5 x 8.8–9.2 μ in South Florida. There is also Cleland's indication that the stipe is gray below, but Cooke (Queensland-type) says it is pallid. It seems therefore that these spurious differences cannot be used for the substantiation of geographic races. Lloyd's *Strobilomyces pallescens* from this country is, no doubt, plain *B. ananas*.

Krieger (The Mushroom Handbook, p. 441. 1936) uses the name *Strobilomyces coccineus* (Fr.) Sacc. for our *Boletellus ananas*. As for the ge-

neric name, I refer to the observations in the present paper (p. 105); the specific name has the advantage of being undoubtedly the oldest possible name having priority over *Boletus ananas*. When Fries described this species, he based his data on Plumier's laconic characterization and somewhat fantastic picture in black and white. Notwithstanding the very inadequate description and the poor drawing, it is hard to imagine what other species it could possibly represent. On the other hand, it has to be remembered that Plumier gives no locality, and all we know is that it was found somewhere in America. Nobody has ever heard of there being any specimens preserved. This is, in my opinion, not enough evidence to unsettle an established name like *B. ananas*, and Murrill is right in referring to *Boletus coccineus* in the list of doubtful species. Murrill also was right in referring *Boletus isabellinus* Peck to *Boletellus ananas* as a synonym. The type specimens at Albany do not leave any doubt as to their identity. If there is the slightest similarity between plate 3 of Kalchbrenner (l.c.) and the fungus painted, this latter, *Secotium excavatum*, cannot be a *Boletellus*, and by no means *B. ananas*. But Hennings who has seen the "originals" flatly declares *Boletus pallescens* and *Secotium excavatum* to be synonyms. Reexamination of the types of the latter will decide if he is right.

EXTRALIMITAL SPECIES

Boletellus porphyrius (Pat. & Baker) Gilbert, Bolets, p. 107. 1931.
Strobilomyces porphyrius Pat. & Baker, Journ. Straits Branch R. Asiatic Soc. No. 78: 72. 1918.

Described from Singapore, this species has been transferred to *Boletellus* by Gilbert because of the indication in the original description "Spores longitudinally striate" and because of the sordid yellowish hymenium. The examination of the type reveals that *Boletellus porphyrius* actually belongs in this genus, and that it can be characterized as a smaller *B. ananas* with umber brown stipe. The spores are the same in both these species. I found them with exactly the same double markings as in *B. ananas* and of the same size: 17.5–21 x 7.5–9.5 μ. Basidia 32–46 x 12–18 μ; hyphae without clamp connections.

Boletellus emodensis (Berk.) Sing., Ann. Mycol. **40:** 18. 1942.
Boletus emodensis Berk., Hook. Journ. Bot. **3:** 48. 1851.
Strobilomyces annamiticus Pat., Bull. Soc. Mycol. Fr. **25:** 6, *pl. 1, fig. 1.* 1909.
Boletellus annamiticus Gilbert, Bolets, p. 107. 1931.

The characters of this beautiful Asiatic species are plainly those of the section *Ananae*, even the peculiar veil of *B. ananas* being well represented. The originals of Hooker's paintings of Sikkim fungi are preserved at Farlow Herbarium, and they show clearly enough that *B. emodensis* is very similar to *B. ananas*. We have not studied the types which are in England, but we have had material from China, province of Kwangsi, and the types of *Strobilomyces annamiticus* Pat. in the Patouillard Herbarium. The latter is doubtless a mere form of *B. emodensis* if not a plain synonym. The

spores of *B. emodensis* are 18–22.5 x 8.3–11 μ with the wings transversely striate, and 1–1.5 μ high. The spores as indicated by Patouillard come from immature material. Immature carpophores of the Chinese collection have the same spores as indicated by Patouillard (though there is a printing error in his description: 10 instead of 20 μ). The basidia are 39–68 x 12–14.3 μ. The cystidia we have seen are smaller than the basidia or equal the basidia and they are smaller than indicated by Patouillard. The lateral stratum of the hymenophoral trama is distinctly divergent. The mycelium of all specimens is very strongly developed, forming a large, sometimes yellow tomentum. This species differs from *B. ananas* in the color of the stipe, and possibly the deeper red of the pileus and of the hymenophore. It also differs from *B. porphyrius* in the colors and the size. Nevertheless, these three species form a closely related group (a stirps) within the section. This stirps is characterized by the marginal veil, large spores with double ornamentation, and scaly pileus. *Boletus squamatus* Berk. is probably another species of this group, if it is not a synonym of *B. emodensis* with somewhat faded colors.

Boletellus obscure-coccineus (Höhn.) Sing. comb. nov.
Boletus obscure-coccineus Höhnel, Sitz.-ber. Akad. Wiss. Wien **123** (1): 88. 1914.

Höhnel, who collected and described this species in Java, mistakenly thought it was especially close to *Boletus versicolor* i.e. *B. rubellus* Krombh. The type specimens preserved in the Herbarium Höhnel (FH) show that it is a *Boletellus* with the hymenophore convex beneath, 8–9 mm. thick, and the spores 15.5–21 x 7.5–8 μ, melleous when mature (not brown) with comparatively thin walls but with distinct, fine, venose, longitudinal ridges. Basidia 30–42 x 8.5–12.5 μ, 4-spored. Cystidia mucronate, about 100 μ long and 14–24 μ broad. Hyphae of the tube-trama hyaline, cylindrical, smooth, without clamp connections. This species reminds one macroscopically of *Boletellus emodensis*, and microscopically of *Boletellus cubensis* and its allies.

Boletellus cubensis (B. & C.) Sing. comb. nov.
Boletus cubensis Berk. & Curt., Journ. Linn. Soc. **10:** 304. 1868.

The spores of the fragment of the Kew type (NY) are 20–22.5 x 7.5–10 μ, and striped with longitudinal low ridges (less than 1 μ high), deep melleous, fusoid, somewhat truncate at the apex, the ridges slightly anastomosing but not transversely veined as in *Boletellus ananas*, with suprahilar depression. This shows that it belongs in the neighborhood of *Boletellus obscure-coccineus*. It may well be that it will be discovered in South Florida or on the Keys since many Cuban species have been found there by the writer.

Boletus lignatilis Berk. & Curt., Journ. Linn. Soc. **10:** 303. 1868.

This species also comes from Cuba, and the spores of the fragment of the Kew type (NY) are longitudinally ridged (ridges up to 0.8 μ high) and the ridges connected by occasional to frequent anastomoses (but not

transversely venose as in *Boletellus ananas* or *B. chrysenteroides*). They are deep melleous and depressed above the hilar end, 18–21.5 x 7.5–9 μ. This is doubtlessly a *Boletellus* of this section, but no new combination is proposed because of the probability that this and the preceding species are identical.

Boletus guadelupensis Pat., Bull. Soc. Myc. Fr. **16:** 177. 1900.

Collected on Guadeloupe, West Indies, this is possibly another synonym (a younger stage) of *Boletellus cubensis* (B. & C.) Sing. The spores of the type (FH) are 14–19.5 x 6.5–8.5 μ, and longitudinally striate from the hilar end to the tip where the striae suddenly end, very few mature (colored) spores remaining smooth or almost so, melleous, the walls browner, with a slight suprahilar depression or applanation; I have not seen the yellow prominent cystidia. According to the original description, this species differs from *B. cubensis* in chestnut brown pileus and appendiculate margin. The former difference may be due to the fact that the specimen dried to that color, but was reddish when quite fresh, as is evident in the dried specimen. This may be explained by the hypothesis that this species represents a younger stage of *B. cubensis* which would also account for the smaller size of the whole carpophore as well as of the spores. There are also minor differences in the description of the surface of the stipe. Nevertheless, I think it is necessary to refrain from transferring *B. guadelupensis* to *Boletellus* as an independent species, as long as no data on the fresh plants are available. Unfortunately the Cuban plants as well as *B. guadelupensis* seem to be rare.

SPECIES IMPERFECTLY KNOWN

Boletus squamatus Berk., Hook. Journ. Bot. **4:** 137. 1852.

I think that this Indian species is another member of the section *Ananae*, but since nothing is known about the spores, further studies are necessary, before a new combination can be justified. Berkeley thinks that his species is allied to *Boletus subtomentosus* and *B. chrysenteron*, but Hooker's original painting reminds one of *Boletellus porphyrius* and *B. emodensis* rather than of any *Xerocomus*.

Sect. **Mirabiles** Sing. sect. nov.

Pileo minute tomentoso vel squarroso, margine secedente sterili; stipite plus minusve reticulato; sporis giganteis, levibus.

Characters of the section: see key (p. 107), and Latin diagnosis above. This is a remarkable section because of the enormously large, smooth spores, and the projecting margin. It has been treated by Singer conditionally in *Xerocomus*, but it actually is close to the remaining sections of *Boletellus*. The type species is *Ceriomyces mirabilis* Murr. There are no representatives of this section known to occur in Florida.

Boletellus mirabilis (Murr.) Sing. comb. nov.
Ceriomyces mirabilis Murr., Mycologia **4:** 98. 1912.
Xerocomus mirabilis Sing., Rev. Myc. **5:** 6. 1940.

The type of this western species has been studied by the writer and compared with material obtained by Alexander H. Smith. As I said in 1942, "The exact structure of the trama could not be stated. If this would be extremely bilateral, generic separation from *Xerocomus* should be taken in consideration." Actually, the trama of the tube walls *is* truly bilateral. The spores are 18–26.3 x 7–10.5 μ, smooth, ellipsoid-oblong to subfusiform or fusiform, yellowish brown (color in mass unknown to me), wall 1–1.8 μ thick, but also thinner in many cases, slightly obtusate at the apex in many cases, with suprahilar applanation; basidia 33.5–39 x 10–14.5 μ, 4-spored; cystidia 60–80 x 11–16 μ, ventricose or ampullaceous or more or less strongly capitate above, or plainly fusoid; hyphae of the truly bilateral trama without clamp connections and divergent in the lateral stratum; papillae of the cuticle consisting of chains of thin (about 8 μ), or thick (10–18 μ) clampless hyphae, in the latter case attenuated toward the septa, brown, about 20–60 μ long between septa, the terminal member occasionally shortened, and rarely spherocystoid. Kauffman (Pap. Mich. Acad. Sci. Arts & Lett. **5** (1925): 115. 1926) redescribed this species from Mt. Hood, Ore. (*Wehmeyer 28130*, MICH). Snell had it from Manitoba, Slip and Snell indicate it from Idaho, the type came from Washington, and Overholts indicates it in Pennsylvania. This latter extension of the area of distribution is surprising, if true.

Boletellus projectellus (Murr.) Sing. comb. nov.
Ceriomyces projectellus Murr., Mycologia **30:** 524. 1938.
Boletus projectellus Murr., Mycologia **30:** 525. 1938; Coker & Beers, Boletaceae, p. 51. 1943.

This species has been found in Virginia and North Carolina, U. S. A. For a good macroscopical description and a colored picture see Coker and Beers, *l.c., pl. 1, fig. 3–4.* The type specimens (*Murrill F 9296*, FLAS), authentic material (*J. N. Couch F 16045*, FLAS) and other North Carolina material (*J. N. Couch 13193*, FH), were compared by the writer and found to be identical. Couch's collections are evidently the basis of the description and plate of Coker and Beers. The anatomical characters of these plants have been found to be as follows: spores (20)–22–30–(32) x (5.8)–8.5–11–(12) μ, smooth, melleous to deep melleous, the walls 1–1.5 μ thick when mature, the upper portion attenuated from the lower third, or the outline more evenly elliptic-oblong to boat-shaped in frontal view, with a slight depression in profile, always more or less acuminate though obtuse at both ends, the inner wall interrupted and the germ pore incomplete since the exosporium is continuous; basidia 23–36 x (10)–12–15 μ, 4-spored, few 2-spored; cystidia fusoid with ampullaceous apex, or lanceolate with obtuse tip, hyaline, (33)–35–70 x 10–17 μ; trama truly bilateral, the mediostratum slightly denser and dilutely colored, the lateral stratum somewhat diverg-

ing (in nearly adult specimens), clamp connections none. *Boletellus projectellus* is one of the representatives of the Agaricales with the longest spores, and belongs to the species with the most voluminous spores of the whole order. It certainly has the largest spores of all boletes.

Sect. **Retispori** Sing. sect. nov.
Sporis breviusculis, reticulatis.

Characters of the section: see key (p. 107) and the Latin diagnosis above. The section is unique in combining spores of the type of *Strobilomyces floccopus* with entirely different macroscopical features. The only species is *Boletellus retisporus* which has not been found in Florida.

Boletellus retisporus (Pat. & Baker) Gilbert, Bolets, p. 108. 1931.
Boletus retisporus Pat. & Baker, Journ. Straits Branch R. Asiatic Soc. No. 78: 72. 1918.

This Singapore species is extremely interesting, and easily identified by the combination of yellow hymenophore and short, reticulate spores. I have studied the type specimen in the Patouillard Herbarium (FH). The spores are asymmetrical, brown, the ridges forming the complete and conspicuous network are 1.3–2 μ high, but often half or entirely imbedded in a colorless outer wall. My measurements are: 12–12.5 x 8–9.5 μ. Basidia 28 x 11 μ. Hyphae 5–12 μ thick in the trama, without clamp connections. The mycelium is white, and the pileus, though this is not mentioned in the original description, is distinctly appressed-squamulose.

Sect. **Chrysenteroidei** Sing. sect. nov.
Pileo haud roseo nec purpurascente nec carmineo, at flavo, fusco, castaneo; sporis longitudinaliter vittatis vel striatis, aut levibus, elongatis, aut latissimis (10.5–11 μ) aut moderate voluminosis (circa 12.5–17 μ longis); velo spurio vel nullo; pileo stipiteque haud viscidis; stipite haud ceraceo-lacunoso.

Characters of the section: see key (p. 107), and the above Latin diagnosis. The type species is *Boletellus chrysenteroides* (Snell) Snell.

KEY TO THE SPECIES

A. Spores consistently smooth. *Boletellus turbinatus*

A. Spores, at least their overwhelming majority, ridged because of longitudinal lamellose wings.

B. Wings of the spores striped transversely by low veins, or, at least, the bases of the wings connected by such transversal veins, giving the spores an appearance reminiscent of the ornamentation of the spores in *Boletellus ananas*. *Boletellus chrysenteroides*

B. Wings not connected nor striped by regular transversal veins.

C. Pores discolorous, not yellow to melleous as the tubes, but red.

D. Spores 15.5–17 x 10.5–11 μ, with the lamellose wings projecting 1–1.7 μ. *Boletellus Linderi*

D. Spores 12.5–15.2 x 6.5–7 μ; wings lower than 1 μ. 6. *Boletellus pictiformis*

C. Pores concolorous, yellow or melleous.

E. Spores (12.2)–13–13.8–(15.3) x (5.5)–6–(6.8) μ. *Boletellus pictiformis* var. *fallax*

E. Spores 14–22.5 x 6.5–10 μ (compare *B. chrysenteroides*, if collected on the North American Continent, and *B. guadelupensis* if collected in the West Indies).

DESCRIPTION OF THE SPECIES OCCURRING IN FLORIDA

6. **Boletellus pictiformis** (Murr.) Sing. comb. nov. var. *typicus*.
Suillellus pictiformis Murr., Lloydia **6** (3): 226. 1943.

Pileus castaneous, densely shaggy-tomentose, convex, 40–60 mm. broad. — *Hymenophore* yellow or melleous with red pores, changing at once to cyaneous when wounded, plane pores 1 mm. in diameter. — *Stipe* lateritious above, castaneous below, very shaggy, sulphureous at the apex, enlarged upward, 60–80 x 10–13 mm. — *Context* citrinous, yellow, turning blue at once; taste mild or acid. (Macroscopical description drawn from W. A. Murrill's notes.)

Spores 12.5–15.2 x 6.5–7 μ, intensely melleous, subfusoid, with longitudinal ridges from the tip downward to the hilar tube, the ridges occasionally anastomosing or forking, but their sides and the space between them not veined transversely; *basidia* 27–34 x 11.5–14.2 μ, 4-spored; *cystidia* clavate-mucronate, or fusoid-ventricose with a cylindric ampullaceous neck which sometimes is curved, or merely fusoid with obtuse apex, hyaline, or with a melleous belt-like incrustation; *terminal hyphae* of the tomentum of the pileus melleous, clavate, 82 x 10.8 μ, the lower hyphae 4–14 μ in diameter, without clamp connections.

Habitat: On banks under trees. July to August.

Distribution: North Florida.

Material studied: Kelley's Hammock, Alachua Co., *Murrill* (FLAS, **type**); also authentic material from Sanchez and Planera Hammock, Alachua Co., *Murrill F 17945*, *F 17862* (FLAS).

The type variety is evidently very rare even in the large hammock region northwest of Gainesville where it has been found exclusively. It is so rare indeed that W. A. Murrill has collected it only four times since 1938, and I was not able to gather specimens during my stay in Gainesville. It is remarkable because of its relation with an African species, *B. Linderi*. These species are very similar in many regards and are no doubt closely related. It is interesting to find two species with striking affinities at two isolated points on opposite sides of the Atlantic. *B. pictiformis* may well be derived from forms with African ancestry, but it would be dangerous to generalize concerning this probability for all Strobilomycetaceae of this hemisphere, since *B. ananas* and *B. porphyrius*, *Porphyrellus subflavidus*, *P. malaccensis* and *P. lacunosus*, and also *Boletellus jalapensis* show quite different geographic relationships, extending rather across the Pacific than across the Atlantic.

Boletellus pictiformis (Murr.) Sing. var. **fallax** Sing. var. nov.

A typo differt poris concoloribus cystidiisque paucis incrustatis.

Pileus brown because of the "Prout's brown," "mummy brown," or "bister" appressed fibrose scales with the pale yellow flesh showing through between them, not viscid, convex, without or with only slightly projecting margin, 42–53 mm. broad. — *Hymenophore* "yellowish citrine" to "olive lake" when mature, with concolorous pores which are angular and number 5–10 per 5 mm. transversely, and individually 0.3–1.5 mm. in diameter, i.e. rather large when quite mature, the tubes somewhat depressed around the stipe, moderately long (5–7 mm.), turning blue on injury. — *Stipe* below "mummy brown," in the middle "madder brown" to "Vandyke brown," or paler and with yellow shining through, and at the apex "Pinard yellow," fibrous-scaly-shaggy in the color of the pileus, the scales often arranged in a coarsely reticulate manner, solid, equal, with acuminate-tapering base, 60 x 12–13 mm. — *Context* of the pileus yellow, richer yellow at the apex of the stipe ("baryta yellow" to "empire yellow"), between "bay" and blackish brown at the base, strongly and readily turning blue in the pileus when cut, rather thick in the pileus; odor and taste not remarkable.

Spores (12.2)–13–13.7–(15.3) x (5.5)–6–(6.8) μ, deep melleous, with low to medium high longitudinal lamellose wings which are not connected by regular transverse veins, but occasionally by oblique anastomoses of the wings which are often forked, with suprahilar depression or applanation, ellipsoid-fusoid; *basidia* 4-spored, 25–39 x 12–13.7 μ; *cystidia* numerous near pores, mostly ampullaceous, pale melleous to hyaline, not incrusted or occasionally somewhat incrusted, (23)–38–60 x 10–14.2 μ; also some extremely versiform cheilocystidia are present at the very edge of the tube walls; *cuticle* of the pileus made up of repent or obliquely ascending strands of parallel, cylindric hyphae with the second-last member always elongate and 4–10 μ thick, the terminal member cylindric with rounded ends and as long as the other hyphae, or shorter, with some rare spherocysts on the very surface.

Chemical reactions: **KOH** on the surface of the pileus eventually deep reddish brown; on tubes and context ochraceous brown. — **NH_4OH** on the surface of the pileus black; on the tubes and on the context negative, or removing the blue stain when applied after autoxydation has taken place. — **HNO_3** on the surface of the pileus rufous; on the context pale reddish-brown. — **$FeSO_4$** on the surface of the pileus negative; on the context negative, or removing the blue stain if applied after the autoxydation has taken place. — **Formol** on the context provoking more blue coloration.

Habitat: Under mixed hardwoods, in a mesophytic hammock. July.

Distribution: North Florida.

Material studied: Kelley's Hammock, N. W. of Gainesville, Alachua Co., July 14, 1943, *Singer F 2716* (FH).

This variety lacks the most characteristic feature of the type, and Murrill did not recognize it as his species when he saw it in fresh condition.

However, the complete identity of the anatomical characters and the fact that the type of var. *typicus* and the type of var. *fallax* were found in the same hammock, and also the agreement of the important macroscopical characters, makes it impossible to consider the variety as a separate species. I have called it var. *fallax* because it suggests other species rather than *B. pictiformis*, e.g. *Boletellus chrysenteroides* (Snell) Snell which, however, has not a floccose-fibrillose-squamose stipe, but only shows furfuraceous punctations. Its spores usually have distinct transverse, regular anastomosing veins between the wings, reminding one of the ornamentation of *B. ananas*. Nothing like this is seen in spores of *B. pictiformis* var. *fallax*. The cuticle of *B. chrysenteroides* and *B. pictiformis* var. *fallax* also are very different in structure.

EXTRALIMITAL SPECIES

Boletellus chrysenteroides (Snell) Snell, Mycologia **33:** 422. 1941.
Boletus chrysenteroides Snell, Mycologia **28:** 468. 1936.

This has been described from New York, but additional material has been found in the Farlow Herbarium from Glenbrook Ravine, northeast Georgia, A. B. Seymour, det. W. H. Snell (*B. fumosipes*), and there is material in E. V. Seeler's Herbarium, collected by him on Nantucket, Mass., det. D. H. Linder & R. Singer. W. H. Snell was kind enough to send me his entire material, including specimens from North Carolina, West Virginia, Tennessee, Missouri, and Pennsylvania, all the same species with the covering of the pileus like that of *Porphyrellus pseudoscaber*, and actually with the same type of structure, but of a different brown. The spores are somewhat variable from specimen to specimen, (9.5)–11–17.3–(18.2) x 5.8–9.8 μ, with longitudinal lamellose wings of 0.6–0.8 μ height, melleous, the wings connected by regular, transverse veins recalling those of *Boletellus ananas* but not always running all through and up the sides of the wings reaching their edges; basidia 4-spored, 25–39 x 11–19 μ; cystidia fusoid to ampullaceous, hyaline or pale melleous, not incrusted, numerous on and near the pores, 34–62 x 9.5–18.5 μ; trama initially truly bilateral (*Boletus*-type), later more regular; epicutis formed by a palisade of erect, septate hyphae, with the last members conic-fusoid or cylindric, the second-last ones conic-obtuse or like the lower ones, i.e. almost as broad as long and rather voluminous, fulvous brown; they measure: last member 17–18 x 5.8–9.8 μ, second 17–18 x 13–17 μ below, and 7–11 μ above, third and following members 17–18 x 13–17 μ; hyphae without clamp connections.

One of the specimens, a co-type of Snell's, collected at Burnet, Quebec, Canada, differs from all other specimens in having the longitudinal wings of the spores more frequently anastomosing, or even with transverse ridges, but not always showing the characteristic thin, low veins found in the type; these spores are a trifle larger, 13–18.7–(20) x 7.3–9.2–(10.2) μ, and thus are intermediate between *B. chrysenteroides* and *B. guadelupensis*; their wings are 0.8–0.9 μ high, some lower. The external appearance of the fruit

bodies is nearly the same as in the type, but they are larger in all parts. Since two small characters are coupled in this case, distinguishing the northernmost collection from all the rest, one may suspect that the Canadian form is a geographic race. Further observations on this subject are needed. *B. chrysenteroides* is apparently the northern equivalent of the Floridian *B. pictiformis*.

Boletellus Linderi Sing. spec. nov.

PLATE I, FIG. 10.

Pileo flavo ("Naples yellow" Ridgway), fibrillulis squamulisque badiocastaneis ("mahogany red" Ridgway) dense obtecto, (25)–60 mm. lato; hymenophoro poris primum rubris et aequalibus, deinde aurantiacis et irregularibus instructo, e tubulis longis consistente; sporis ellipsoideis, brunneis, 17–23–alatis, ornamentatione 0.9–1.7 μ alta, polo apicali levi, 15.5–17 x 10.5–11 μ metientibus; basidiis 34 x 14.7 μ, clavatis, tetrasporis; cystidiis circa 63 x 22 μ, ventricoso-ampullaceis; stipite ad apicem luteo ("light cadmium" Ridgway) et atro-squamoso, fibroso, elongato; carne fracta caerulescente. Habitatio: Ad folia delapsa ad basin arboris Augusto mense. In Liberia Africae Occidentalis, prope Miamu. *D. H. Linder 408* (FH).

Boletellus turbinatus (Snell) Sing. comb. nov.

PLATE I, FIG. 3.

The type from Missouri (FH) shows the spores to be deep honey color with a fulvous tinge in the episporium, smooth but thick walled, the apical portion attenuate and mostly subacute, often with an incomplete germinative pore. These are typical *Boletellus*-spores in spite of their being smooth. I found them 13.5–20 x 5.5–9.5 μ; Snell finds them up to 21 μ long. I suspect that the turbinate habit is merely incidental.

SPECIES IMPERFECTLY KNOWN

Boletus chrysenteron Bull. *sensu* Coker & Beers, The Boletaceae, p. 70, *pl. 44* (below), *pl. 64, fig. 8.* 1943 (*non* Fr.).

Discovered in North Carolina, this species agrees with the preceding one in many ways, but differs in the more truncate apex of the spores and the spore measurements as given by Coker and Beers. Material from Chapel Hill, however, has the spores larger than given by these authors, viz. (11.5)–13–17–(26) x 4.8–6.5 μ. This cannot be Bulliard's plant, since species with spore characters like those described and figured by Coker and Beers do not occur in the environs of Paris. It is also not Fries's *Boletus chrysenteron* which equals *Boletus communis* Bull. *sensu* Coker & Beers (*l.c.*, p. 62). This latter name can not be accepted according to the rules of nomenclature. If *Boletus chrysenteron sensu* Coker & Beers should turn out to be different from *Boletellus turbinatus*, it must be renamed. However, we are inclined to think that the turbinate shape of the latter is an accidental character of no taxonomic value and little if any constancy; consequently these species may be synonymous.

Sect. **Ixocephali** Sing. sect. nov.

Superficiebus sterilibus carpophororum plus minusve viscidis; sporis longitudinaliter vittatis; stipite haud subceraceo-lacunoso.

Characters of the section: see key (p. 107) and Latin description above. The type, *Boletellus singaporensis* (Pat. & Baker) Gilbert (*Boletopsis singaporensis* Pat. & Bak.), has a distinct annulus, but I think that evelate forms like *Boletellus jalapensis* are close enough to be placed in this same section. The section *Ixocephali* is not represented in Florida.

Boletellus singaporensis (Pat. & Baker) Gilbert, Bolets, p. 107. 1931.
Boletopsis singaporensis Pat. & Baker, Journ. Straits Branch R. Asiatic Soc. No. 78: 69. 1918.

The species has a beautiful, distinct, tardily lacerate annulus which is said to be viscid in fresh condition. The type (FH) shows a pretty species with long stipe and thick hymenophore, well agreeing with the original description. The spores are comparatively short, 14–15 x 10.5–12.5 μ, with about 13 longitudinal wing-like lamellose ridges which are brown on melleous ground, and 1.2–1.6 μ high, and suddenly cut off at the apex, with a globose oil-drop and asymmetrically inserted hilar tube, some of the ridges not reaching the opposite end, not transversely veined, wall 0.8–1.1 μ thick; basidia 23–27 x 15–17 μ; cystidia subulate-fusoid with rounded tips, 45 x 10.5 μ. This species, under a temporary herbarium name, is represented in Patouillard's Herbarium (*#4992*).

Boletellus jalapensis (Murr.) Gilbert, Bolets, p. 107. 1931.
Ceriomyces jalapensis Murr., Mycologia **2:** 248. 1910.

The type (NY) from Mexico has 16–18–(25) x 10–11–(13.5) μ large, deep melleous spores with 10–14 longitudinal ribs with scarce anastomoses but usually once or twice forked ribs among them, the ribs 1–1.5 μ high; basidia 38–42 x 14–15.5 μ, 4-spored; cystidia not found but probably present; hyphae without clamp connections; structure of the cuticle of the pileus not clear, hyphae clampless, elongate, some claviform, some very thin, yellowish-subhyaline, of variable diameter.

I have not seen the material determined as *Boletogaster jalapensis* by Lohwag but judging from the picture and the description, it may be a long-stemmed form of this species. This would extend the area of *B. jalapensis* to South China, another indication of the close relationship between the boletineous flora of North and Central America and Eastern Asia. It may, however, turn out that Lohwag's determination was only approximate, and that the Chinese fungus is a third species of this section characterized by its stipe which is twice as long as in typical *B. jalapensis*. As for the description of the ornamentation of the spores, I am not sure that Lohwag's observations are quite exact. If they are, it would indicate an additional difference between this and the Chinese species. The spores of the Mexican type have the projecting angles (corresponding to the ribs) when seen from above, *not* semicircular but lamellose-rectangular, thinner than the distance

they project from the surface of the wall proper, and their number, when counted at one of the poles never reaches twenty.

Sect. **Dictyopodes** Sing. sect. nov.
Pileo tomentoso, haud viscido; sporis longitudinaliter costato-vittatis, elongatis; stipite subceraceo-lacunoso et ex ea re reticulato vel longitudinaliter lamellato.

Characters of the section: see key (p. 107) and the above Latin diagnosis. The type species is *Boletellus Russellii* (Frost) Gilbert (*Boletus Russellii* Frost).

7. **Boletellus Russellii** (Frost) Gilbert, Bolets, p. 107. 1931.
Boletus Russellii Frost, Bull. Buffalo Soc. Nat. Hist. **2**: 104. 1874.
Ceriomyces Russellii Murr., Mycologia **1**: 144. 1909.
Boletogaster Russellii Lohwag *in* Handel-Mazzetti, Symb. Sin. **2**: 56. 1937.
Frostiella Russellii Murr., Mimeogr. Contr. Herb. Univ. Fla. Agr. Exp. Sta. Gainesville, p. 6. 1942.

PLATE I, FIG. 9.

Pileus "Sayal brown" and "wood brown" mixed, later about "avellaneous," strongly tomentose, becoming strongly furfuraceous to felty-squamulose, dry or scarcely subviscid after prolonged rains, with smooth margin which strongly projects beyond the hymenophore, appearing lobate or crenate from above, pulvinate, 35–80 mm. broad. — *Hymenophore* "naphthaline yellow," becoming "barium yellow" to "strontian yellow," pores concolorous but with a slight pinkish hue when young, surrounded by the sterile projecting margin which is pallid and reddening, tubes medium long, 5–9 mm., adnate, becoming depressed around the stipe when mature, pores rather small to medium wide, widest in the marginal row, 3 per mm., then becoming wider, most frequently 0.8 mm. wide, but sometimes up to 1 mm. in diameter; spore print "dark olive." — *Stipe* "cacao brown" to "walnut brown," at least above, the ribs and mostly also the entire base "vinaceous pink" to "buff pink," with very high (about 2–3 mm.) lamellose longitudinal ribs which strongly anastomose, making the surface appear conspicuously lacunose-favose, the single honeycomb of varying length (1–10 mm.), the base usually smooth but often covered with white to whitish yellow mycelial felt, solid, becoming slightly hollow, slightly to strongly viscid outside, especially below, tapering at the base into the ground, and also tapering upward in the upper one or two thirds, or thickened at the base and gradually tapering to the apex, the basal portion usually covered by rotten leaves, 63–80 x 14–20 mm. — *Context* whitish to creamy pallid, stramineous-pallid, becoming eventually melleous in the base of the stipe, and reddish under the cuticle, more so when wounded, eventually almost "walnut brown" in the upper portion of the pileus, almost unchanging on injury, rather hard in the stipe but the consistency of the ribs almost waxy, soft-fleshy in the pileus; taste mild; odor almost none, eventually slightly mawkish.

Spores 16–19.5 x 8–11 μ, melleous, ornamentation and wall brown, with longitudinal, wing-like, lamellose ridges which run from the apical pole to

the hilar tube, suddenly stopping at the apex, without the transverse veins of *Boletellus ananas*, occasionally some ridges connected by a few scattered anastomosing ridges, the longitudinal ridges numbering about 13 at the poles, and projecting 1.8–2.1 μ; *basidia* 25–35 x 13–16 μ, 4-spored; *cystidia* mostly fusoid-ampullaceous, often compressed, 45–70 x (6.5)–9.5–15 μ, occasionally with a colored incrustation but wall and contents always hyaline; *trama* of the hymenophore truly bilateral, mediostratum slightly darker and denser than the strongly diverging hyaline lateral stratum; *cuticle* of the pileus consisting of multi-septate, erect chains of hyphae with clampless septa, most members of these chains rather short to almost subglobose, but the terminal cell elongate (about 50 μ long), cylindric or subulate; surface layer of the stipe consisting of a palisade of dermatocystidia and abundant dermatopseudoparaphyses, also some basidia; the *dermatocystidia* of the young specimens hyaline to brown, ampullaceous, up to 80–100 x 10.8 μ; *dermatopseudoparaphyses* up to 11 μ broad; *basidia* 4-spored, about 46 x 13.2 μ; *hyphae* of the context without clamp connections.

Chemical reactions. **KOH** on the surface of the pileus chestnut or fulvous; on the context of the upper portion of the pileus brown; on the context of the stipe slowly brownish; **NH_4OH** on the surface of the pileus somewhat deeper and more brick colored; on the context of the upper portion of the pileus red; on the context of the stipe slowly reddish; **$FeSO_4$** on the surface of the pileus deep olive; on the context of the pileus and stipe negative; **formol** negative; **phenol** negative.

Habitat: Under oaks, in Florida under *Quercus laurifolia* in high hammock formations on the soil amongst leaves. Summer.

Distribution: All along the Atlantic Coast from New England to Central Florida and west to Wisconsin (according to Murrill) and Mississippi. In Florida not reaching the tropical part of the peninsula.

Material studied: **Fla.:** Alachua Co., in and near Gainesville, *R. Singer F 2500, F 2500a* (FH); Lake Co., Eustis, on wood of *Sabal, R. Thaxter* (FH); — **Vt.: type** of *Boletus Russellii*, Brattleboro, in damp woods, 1865, *C. C. Frost* (FH). — **N. H.:** Shelburne, Aug. 1891, *Farlow* ("form with scaly top") (FH); Chocorua, September 1917, *Farlow* (FH); Intervale, August 1901, *Thaxter 3473* (FH). — **Mass.:** Arlington, *H. M. N.* (FH): Middlesex Fells, August 1897, *Farlow* (FH); Canton, August 1925, *D. H. Linder* (FH). — **N. C.:** Glassy Mts., Flat Rock, August 1934, *A. S. Rhoads* (det. W. A. Murrill) *F 9385* (FLAS).

B. Russellii is unmistakable for anyone who has once collected it. Its history shows clearly that there was doubt about its specific taxonomy only once when Lloyd claimed its identity with *B. betula*, an error subsequently corrected by W. G. Farlow. There was, however, considerable fluctuation in its generic position. I cannot see any reason for splitting the genus *Boletellus* into several genera, but if it is done (which would make it necessary to go much farther in breaking up the genus than we do now in dividing the boletaceous genera and the agarics) it should be done according to the sections outlined in the key p. 107. However, *Frostiella* is not a

valid name since it has been published without a Latin diagnosis, and in the sense of Murrill covers several sections of *Boletellus*; thus it becomes a synonym of the older name *Boletogaster* Lohwag. There is a fine colored picture in *Icones Farlowianae, pl. 80*, and a good photograph has been published in Coker and Beers, *The Boletaceae, pl. 48*. MacIlvaine *pl. 118, fig. 2* is likewise correct. Elrod and Blanchard (Mycologia **31:** 701, *fig. 2, E.* 1939) published a photomicrograph showing the structure of the trama. The difference of the picture obtained as compared with our results is due to insufficient consistency on the part of these authors in their choice of corresponding and comparable stages of development. In young hymenophores, the trama of practically all Boletineae is more or less bilateral, and all Strobilomycetaceae have the truly bilateral trama of the *Boletus*-type.

SPECIES NOT KNOWN TO THE AUTHOR

Boletellus costatus (Rostrup) Sing., Ann. Myc. **40:** 18. 1942.
Boletus costatus Rostrup, Botan. Tidskr. **24:** 357. 1902.

Described from Thailand, tropical Asia, this probably belongs in this section.

Sect. **Allospori** Sing. sect. nov.
Pileo viscido; sporis spinis exosporio immersis ornatis; stipite subceraceo-lacunoso et ex ea re reticulato vel longitudinaliter lamellato.

Characters of the section: see key p. 107, and Latin diagnosis above. The type species, *B. betula* (Schw.) Gilbert, is so far the only species of this section. It has not been found in Florida.

Boletellus betula (Schw.) Gilbert, Bolets, p. 108. 1931.

A North American species, reported from many eastern and middle western states, has also been called *Ceriomyces betula* Murr., *Frostiella betula* Murr., and *Boletus Morgani* Peck. It was initially described as *Boletus betula* by Schweinitz, Schr. Naturf. Ges. Leipzig **1:** 90. 1822. I have studied specimens from several localities, and the results of my microscopical analyses follow:

Spores 16–21.5 x 8.5–10 μ, deep ochraceous-melleous, with suprahilar applanation, ellipsoid-fusoid, with isolated punctiform warts if the upper surface of the spore is focussed upon, but actually the wall perforated by cylindric thin spines which are darker than the wall and hardly pass beyond its outer surface; basidia 25–40 x 11.6–16.5 μ, 4-spored; cystidia present; the pellicle consists of a palisade of dermatocystidia (100 x 8 μ) and more basidium-like bodies; the surface of the stipe consists of a palisade of sterile elements.

SPECIES INCERTAE SEDIS

Strobilomyces paradoxus Mass., Bull. Misc. Inf. Bot. Gard. Kew **1909** (5): 209. 1909.
Boletellus paradoxus Gilbert, Bolets, p. 107. 1931.

This species has been described from Singapore. I have seen no specimens.

Boletus Rostrupii Syd., Ann. Myc. **1:** 177. 1903.
Boletus lacunosus Rostr., Bot. Tidskrift, **24:** 357. 1902 (non Cooke).

This species has been described from Thailand. I have seen no specimens but it seems reasonable to assume that it belongs in the Strobilomycetaceae.

Strobilomyces fasciculatus Cooke, Grevillea **20:** 4. 1891.

This species from Australia may or may not belong in the Strobilomycetaceae. No specimens have been examined by the writer.

Strobilomyces ligulatus Cooke, Grevillea **20:** 4. 1891.
Boletellus ligulatus Sing., Ann. Myc. **40:** 18. 1942.

Described from Australia, this seems to be a *Boletellus*, provided the description is correct. However, the spore ornamentation (if there is any) has not been mentioned by Cooke.

Strobilomyces rufescens Cooke & Mass., Grevillea **18:** 5. 1889.

The description of this Australian species reads much like that of *Strobilomyces pallescens* which is a synonym of *Boletellus ananas*.

Boletellus pustulatus (Beeli) Gilbert, Bolets, p. 107. 1931.
Boletus pustulatus Beeli, Bull. Soc. Roy. Bot. Belg. **58:** 211. 1926.

Described from Belgian Congo, this species has been transferred by Gilbert to *Boletellus*, and, judging from the original description, correctly so. If the spores actually are as small as indicated by Beeli, one would expect this species to belong in the section *Chrysenteroidei*, though its color is more suggestive of section *Ananae*.

Phyllobolites Sing., Ann. Myc. **40:** 59. 1942.

No representatives occur in Florida.

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EXPLANATION OF PLATE I

All drawings are made with the aid of a camera lucida from dried material mounted in KOH-phloxine (basidia and cystidia) or either ammonium hydroxide or Melzer's reagent (spores) under oil immersion lens. Magnification approximately x 1050.

1. *Strobilomyces confusus* Sing. Spores, basidium; from the type specimen.
2. *Strobilomyces floccopus* (Vahl ex Fr.) Sacc. Spores, basidium; specimen from Harvard, Mass.
3. *Boletellus turbinatus* (Snell) Sing. Spores, basidium; from the type specimen.
4. *Porphyrellus subflavidus* (Murr.) Sing. Spores; from authentic material.
5. *Porphyrellus gracilis* (Peck) Sing. Spores, basidium; from authentic material.
6. *Strobilomyces velutipes* Cooke. Spores, basidium; from the type material.
7. *Boletellus ananas* (Curt.) Murr. Spores, basidium; specimen from North Florida.
8. *Strobilomyces pterosporus* Sing. Spores, basidium; from the type specimen.
9. *Boletellus Russellii* (Frost) Gilb. Spores, basidia and cystidia.
10. *Boletellus Linderi* Sing. Spores, basidium; from the type specimen.

PLATE I